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INTRODUCTION

1. While reading linguistic literature, one is constantly confronted with various kinds of units, such as languages, sentences, words, etc. Terms like these are very convenient tools in linguistic discussions, their usefulness is proved by long years of practice. Nevertheless, their meaning is for the most part vague, and every now and then linguists stop to think over the exact delimitation of such notions. The number of definitions proposed since the time the science of language came into existence, is extremely large. These attempts are only partly dictated by the wish to deepen the theoretical insight into the subject. Of no less importance is the quandary the linguist finds himself in when his assistance is called in for a concrete solution in a case of doubt. He may be asked, for example, whether Dutch and Frisian, or Danish and Norwegian, or Russian and Byelorussian are separate languages or merely varieties of one language; or his judgment may be invoked in the question whether it is linguistically correct to write *fish ball* in two words, but *football* in one. It often happens that a specialist, no less than a layman, appeals to considerations which, strictly speaking, lie outside the field of linguistics proper, such as the national feelings of the members of a given community or spelling conventions. The question arises whether a linguist should be satisfied with this state of affairs.

The situation is more disagreeable when a linguist comes across a problem of this kind during his own work. Actually, several types of linguistic investigation place the student before the need of a solution. In order to produce a synchronic description of a language in accordance with the trends now prevailing, it is necessary to operate with some definition of what a language is, and this definition must not only be roughly feasible, but narrow

enough to exclude all cases of doubt as to whether a given element does or does not belong to the language under study. The reason is that modern linguistics starts from the conviction that a language is a system where everything is related to everything else. Naturally, one can state a relationship only then when the presence of the elements involved is an unmistakable fact.

There exists a commendable tendency to regard a language as a body of texts. As long as a language has not died out, this body of texts will be of infinite length. (It goes without saying that the term "text" is to be understood here not only in the sense of written records of a language, but also, and in the first place, of spoken utterances.) So our question appears in a new form: when may two or more utterances be considered representative for one and the same language? The problem can be made less insurmountable by studying the speech of only one informant and using it as a touchstone for the validity of other testimonies. But such a procedure only diminishes the obstacle, it does not remove it, for not all utterances of this one speaker may be equally correct: anybody produces once in a while a linguistic miscarriage.

For this reason the linguist uses his informant afterwards as the interpreter of his own utterance. He checks with him his records and has him indicate those sentences which he regards as wrong. It is clear that the same informant can also interpret the texts obtained from other informants. He may recognize a text as belonging to his own language, but nevertheless find in it some forms instead of which he himself would have used other forms. At this point the same problem reappears: How many deviations can one accept and still speak of the same language? Is there any criterion for measuring the admissible range of variation? It is obvious that the judgment of a native speaker concerning sameness of language has no objective value.

Sometimes the facts are more complicated than those described here. When informant A singles out some forms of a text recorded from informant B as deviating from his own norms, it is by no means excluded that informant B himself would also, when asked, declare that these forms are not in accordance with his own

everyday practice; he may have chosen them for stylistic purposes, because he wanted to emphasize, for example, the folkloristic character of the narration. When it happens that B has a preference for archaic forms, a superficial comparison of the texts produced by the two informants A and B will reveal slight differences between their language, whereas in reality it is completely identical. To both speakers the same forms sound archaic, but A's different literary taste makes him avoid them.

Since similar phenomena may be observed throughout the system of a language, we conclude that, while the linguistic identity of texts is difficult to establish, the identity of persons as to their language system can be proved in an objective way. This latter identity can be deduced from the identical interpretation of texts. So we arrive at a definition: A group of persons interpreting in the same way all linguistic utterances is a linguistic community. A language, then, might be defined as the whole of rules according to which the members of one linguistic community build their own texts and interpret those of others. Perhaps it is preferable, however, to use the term "code" instead (as is indeed done by some scholars) and leave the terms "language" and "dialect" to their nontechnical acceptances. Notions like "the Dutch language" are rather of a sociological than of a linguistic nature, although the percentage of Dutch speakers using the same code, is gradually increasing through the influence of the modern means of communication.

The modern term "idiolect" denotes "the total set of speech habits of a single individual at a given time".¹ In this sense the notion is not yet entirely linguistic, for setting apart individuals is not a linguistic procedure. Therefore I would propose to draw in a linguistically amorphous mass of people only the linguistically relevant borderlines. If we can agree on employing the term "idiolect" in the sense of "code" defined in the preceding paragraph, it will be of great value to the analyzer.

¹ U. Weinreich, "Is a structural dialectology possible?", *Word*, X (1954), pp. 388-400. See p. 389.

Breaking an idiolect further down into styles, as some scholars require (according to Weinreich ¹), makes no sense, for styles presuppose each other. An element cannot be stylistically marked unless there is an otherwise identical element which is stylistically neutral. The coexistence of such elements within the same encoding and decoding mechanism is an indispensable condition for stylistic features.

I fully agree with Jones, who, after a brief survey of functional and structural approaches to the sounds of languages, goes on: "It may be added that I find all the attempted definitions of the phoneme to be unsatisfactory on account of the apparent impossibility of knowing precisely what is meant by 'a language'. The term 'a language' is a vague one with psychological or superphysical connotations. It may presumably be said to mean the total material upon which a person can draw when he speaks. But where is that material? It surely 'exists' only in some mentalistic or non-material sense." ² I do not believe, however, that this paragraph contains an argument against functionalism. On the contrary, the functionalistic standpoint should enable the investigator to submit any material to a test which furnishes reliable information on the language. I consider the absence of such a test in the teachings of Jones a definite disadvantage: the judgment of a native is too rarely involved. The difficulty is not entirely smoothed out by the requirement that "a 'language' is to be taken to mean the speech of one individual pronouncing in a definite and consistent style", ³ for the demand that an informant produce only "data of a uniform style" ⁴ is unreasonable. If it is supposed to be the task of the investigator, however, and not of the informant, to ascertain the style of every single bit of material, we must not forget that he can do so only after having completed his analysis of the language,

¹ U. Weinreich, "Is a structural dialectology possible?", *Word*, X (1954) p.p. 388-400. See p. 389.

² D. Jones, *The Phoneme: Its Nature and Use* (Cambridge, 1950), § 658, pp. 216f. Compare also § 626, p. 204.

³ Jones, *The Phoneme* . . . , § 28, p. 9.

⁴ K. L. Pike, *Phonemics. A Technique for Reducing Languages to Writing* (Ann Arbor, 1954), p. 143.

and therefore he is unable to follow Jakobson and Halle's advice: "When analyzing the pattern of phonemes and distinctive features composing them, one must resort to the fullest, optimal code at the command of the given speakers",⁵ for the "optimal code" is not yet known at that moment.

A person's interpretation of a bit of material as stylistically colored characterizes this person as the carrier of a certain language system, but the occurrence of the same element in a text does not say anything about the language of the speaker.

There are three reasons because of which it is advisable to draw our information about a language from a speaker's passive use of it, his decoding habits, rather than from his own production of texts. Firstly, we can have fuller knowledge about the physiological and physical properties of the examined text. Secondly, the informant has fewer chances of making mistakes when he selects the stylistically colored forms from a text offered to him than when he is asked to speak consistently. Thirdly, "a consistent use of the forms of natural, unstudied, but reasonably careful and not rapid conversation", as required by Jones (p. 9), supplies us only with the stylistically neutral forms, so that a large part of the language material does not reach the investigator, unless he asks the informant afterwards to speak consistently in all other styles.

If we admit, with Sapir,⁶ that "all significant entities in experience are [...] revised from the physically given by passing through the filter of the functionally or relatedly meaningful", we understand that languages are differentiated by the shape of their respective filters, and that we must study the filters in order to get an insight into the languages. We can do so starting from Sapir's supposition: "If the phonemic attitude is more basic, psychologically speaking, than the more strictly phonetic one, it should be possible to detect it in the unguarded speech judgments

⁵ R. Jakobson & M. Halle, *Fundamentals of Language* (The Hague, 1956), p. 6.

⁶ E. Sapir, "The Psychological Reality of Phonemes" (1933), reprinted in *Selected Writings of Edward Sapir in Language, Culture, Personality*, ed. by D. G. Mandelbaum (Berkeley-Los Angeles, 1949), pp. 46-60. See p. 46.

of naïve speakers who have a complete control of their language in a practical sense, but have no rationalized or consciously systematic knowledge of it" (p. 47). Or, in Hockett's words, "knowing input and output, one can describe the code".⁷

2. Connected with the approach to language as a closed unit is the recognition that one type of operation is fundamental in linguistic analysis, to wit the operation which consists in resolving a continuum into separate units. So the sound continuum of a language has to be analyzed into units of sound, such as phonemes. A linguistic description of the formal side of a language has to register these units.

When we speak of texts or utterances, it must be borne in mind that these are complex linguistic signs and thus contain not only a form, but also a meaning. Both form and meaning are part of the linguistic sign. Here a similar operation of deriving units from a continuum has to be executed as an essential part of linguistic analysis. The meaning of a word is its possibility to point to certain things in reality (in the same way as the form of a word is its possibility to be recognized). Reality presents itself to us as a continuum, but we are trained, as speakers of a language, to make certain divisions in this continuum. A well-known example is the case of the colors: the spectrum is a continuum in which every language community draws in its own way borderlines, so as to get units, for only in the shape of a unit an element of meaning can fuse with a unit of form into a sign. The way the continuum of meaning is analyzed in a given language, characterizes a language no less than does the way this operation takes place in the continuum of sound.

The parallelism of these two operations, on the plane of the sound and on the plane of the meaning, cannot be sufficiently stressed. A similar experiment with two informants – one speaking a language A, the other B – can be performed on both planes. When asked to respond to two flows of sound [pat] and [pät], A may find that they represent different pronunciations of the same word,

⁷ Ch. Hockett, *A Manual of Phonology* (Baltimore, 1955), p. 190.

but B may clearly notice a borderline between them and perceive two different words. Likewise, when confronted with two shades of color, A will interpret them as varieties of the same color, say green, whereas B will regard them as two separate colors, say green and blue.

Theoretically, a structural scrutiny of a language may start from either experiment, the formal and the semantic one. In practice, however, showing a certain reality to an informant and having him interpret it in terms of his language, does not lead to the desired result, for the meaning of an utterance comprises more than the perceptible world: an intrinsic part of any linguistic meaning is the attitude of the speaker to the reality he handles. Thus modern linguistics is right in working from sound to meaning, and not in the opposite direction.

In the present study the point of departure is the conviction that the possibility of drawing a sharp line between two elements (of sound or meaning) is a *sine qua non* for ascertaining these elements as relevant units of the language. Language is in the first place a means of communication, a sign system, and no such system would work without sharp distinctions. Categories which are not clearly demarcated, are irrelevant for this most important aspect of language.

Briefly, the reasoning starts from the assumption that there are always two facts immediately accessible for the inquirer: the utterance and the interpretation of it by a native. The interpretation depends on two things: the utterance (the input) and the set of rules applied to it (the code). Thus, knowing both utterance and interpretation, one can draw conclusions about the code. Any distinct change in an utterance that is accompanied by a change in the interpretation cogently points to a rule in the code. This is the basic idea of phonemic – or, in a wider sense, functional – thinking in linguistics.

A consequence of this approach will be that not only the utterances which are recorded from the speech of natives, but also artificial utterances, especially constructed for this purpose by the investigator, may be laid before the informant. His interpre-

tation will yield data on the code, additional to those won by the use of real sentences. It need not be stressed that the most satisfactory results will be obtained when the linguist is his own informant, but in practice such ideal situations are rare.

Very often no native can be consulted. When we are compelled to work with written records, a translation can only partly make up for the lack of an informant. In those cases we can only surmise how a native would react in every single instance. This is commonly done. When we want to know, for example, whether the voiced character of the final consonant in English *red* is relevant or subject to neutralization in this position, we may look for analogous words such as *let*, *bet*, and arrive thus indirectly at the conclusion that *red* is phonemically different from a nonexistent word *ret*, and distinguished from the latter precisely by the feature of voice. This would be exactly the answer of an informant, when we would confront him with two forms [red] and [ret] (provided, of course, that he understands our question rightly).

In this way we are no longer obliged to rely too much on the nonoccurrence of a certain form or type of forms. As a matter of fact, a nonoccurrence supplies us only with an indirect indication about the code, for it may be entirely accidental. A nonoccurring form presented as an "input" to a decoder, however, reveals unambiguously a property of the code.

It has to be studied carefully what kind of judgments may be obtained from native informants so that they can serve us as trustworthy data. In general we can say that only those questions should be posed to the informant which require an application of the rules of the code. He must be asked to perform a part of his habitual speech act – only more consciously than he is accustomed to – to wit the part that consists of the interpretation of an utterance offered to him, but all questions should be avoided which would invite him to rely on his common sense, his education, his intuition, or whatever other nonlinguistic considerations there may be.

PARADIGMATIC DELIMITATION OF PHONEMES

1. In his novel *Point Counter Point*, Aldous Huxley introduces a certain Mr. Quarles, who has a particular way of speaking. He says, for instance: "You've no idyah how exasperating these trivial little things can be, when you've got something more syahrrious and important to think about." Referring to this person, one might say: "Mr. Quarles had more syahrrious things to do." If the listener understands this rightly, he will infer that the speaker not only informs him of something, but at the same time wants to characterize Mr. Quarles, or at least one of his speech habits. A superficial conclusion might be that this sentence transmits a different meaning from that which would be expressed by an otherwise identical sentence with the normal pronunciation of *serious*. Continuing this train of thought, one might find that a difference of sound goes hand in hand with a difference of meaning and, consequently, that this difference of sound is phonemic.

It may be argued that the forms *serious* and *syahrrious* belong to different idiolects. But this is exactly one of the facts a linguist wants to learn from his investigation. He cannot suppose the outcome of his analysis to be known before he starts it. Moreover, if we submit both forms to the judgment of *one* decoder, it is clear that, as to the decoding rules, only *one* idiolect is involved.

Furthermore, it may be objected that the meaning of the forms *serious* and *syahrrious* is the same. We must reckon, however, with the widespread conviction that there are no synonyms in a language. This view leaves the decision – in cases of a large degree of semantic similarity close to identity – entirely to the form. On the other hand, if the aversion to the idea of synonymy is unjustifiable, it means that identity of meaning (as supposedly in *serious* and

syahrinous) does not point unequivocally to the identity of the forms, so that the question remains once more undecided.

Nevertheless, the reasoning which leads to positing a phonemic difference between the two forms, is incorrect, for it overlooks three important points, which make it incompatible with the view that a language is a code.

The first reason why this analysis has to be rejected is a very simple one. Since the pronunciation *syahrinous* is an imitation, there exists a direct, nonarbitrary, connection between the phonetic peculiarity and the alleged meaning, so that sign function does not play a part in it. In this form, the argument is not yet conclusive, because in other cases a mere imitation may result in a phonetic difference which is doubtless phonemic. This happens, for example, when one says [ægen] in a milieu where [ægein] is the normal pronunciation, or *tomato* rhyming to *potato* where the usual stressed vowel in this word is [a:]. The choice of these special forms may be determined by the wish to characterize a person to whom one is referring. The phonemic relevance of the sound differences is deduced in these cases from the fact that they convey a lexical difference of meaning in other pairs of words in the same language, like *raid* : *red* or *pace* : *pass*. A conclusion is reached through the application of the rule: two sounds which are functionally different in some word pairs, are phonemically distinct in other word pairs, too, unless there are clearly demonstrable conditions for a neutralization. Thus we have here an example of the current procedure mentioned in the Introduction (p. 14): indirect indications are sought which make it probable that a native decoder will interpret the words as distinct entities, although their meaning is the same. When we find a reliable way of directly interrogating a native, this will of course be a more efficient procedure. More important is that the criterion of arbitrariness does not work in all cases of seemingly distinctive sound differences, and that it eliminates from the system some sound properties which should be retained, as will be shown below (p. 54).

The second argument against the incorporation of the relationship *serious* : *syahrinous* in the code is that the distinguishing sound

feature presents "singleness of reference" (Sapir), "possession of a single specific denotation" (Jakobson): wherever it occurs it points always to the same peculiarity. The opposite is, in Jakobson's terminology, "emptiness", "mere denotation of otherness",¹ which sets apart the distinctive features from all other sound features. This criterion has the same kind of shortcoming as the preceding one: limited applicability and in other cases a too rigid selection.

The third reason (in my opinion, the most impressive one) why a phonemic distinction of *serious* and *syahrious* clashes with the notion "code", is the demand of "discreteness of the elements", which any part of the code has to meet. It is opposed to Sapir's "grading gamut". The principle has been clearly stated by Hockett: "By discreteness of contrast we mean that no phonologically relevant contrasts are of the continuous-scale type." "In general, then, if we find continuous-scale contrasts in the vicinity of what we are sure is language, we exclude them from language (though not from culture)." ² In my opinion, this is the most useful criterion, the only one which covers all cases and sifts them properly.

2. A correct appreciation of the criterion of discreteness forces us to reexamine the fundamentals of phonemic analysis, for it is closely connected with the way in which phonemes and distinctive features are ascertained. A crucial point for anyone who wishes to set up the inventory of phonemes for a given language is the problem how to establish the identity of phonemes in different environments, in concreto: how can we prove that the *b* of *beak* and the *b* of *cab* are occurrences of one and the same phoneme? The necessity of conquering this problem at some moment influences the choice of the analytic procedure from the very start.

Martinet recommends the method "based on the comparison

¹ R. Jakobson, C. G. M. Fant & M. Halle, *Preliminaries to Speech Analysis. The Distinctive Features and their Correlates* (Cambridge, Mass., ² 1952), p. 14; Jakobson & Halle, *Fundamentals* . . . , p. 11.

² Hockett, *Manual* . . . , p. 17. Compare also *Psycholinguistics. A Survey of Theory and Research Problems*, edited by Ch. E. Osgood and Th. A. Sebeok (Baltimore, 1954), p. 75: "It seems to be possible to distinguish linguistic features as discrete and quantized in contrast to the continuity of the non-linguistic."

of texts with minimal differences".³ Elsewhere⁴ he calls the technique "commutation", a term borrowed from Hjelmslev.⁵ This is the procedure currently followed in phonemic analysis. According to it there are three successive steps. The first step leads to the enumeration of the stock of phonemes occurring in every single environment, Twaddell's micro-phonemes.⁶ We establish one micro-phoneme *b* through a comparison of *beak* with words like *peak*, another micro-phoneme *b* through a comparison of *cab* with words like *cap*, etc. The second step consists of the analysis of the phonemes into their distinctive qualities. The third step is the identification of phonemes in different environments on the basis of their "sets of permanent distinctive qualities".⁷ These are Twaddell's macro-phonemes.⁸

Though this method is widely accepted there exist largely different theoretical interpretations of it. I give two examples to show how differently the relationship between the first and the second step is understood.

First I quote Jakobson: "... as the phonemes of a given language form a system of sequences, so the system of phonemes, in turn, is formed by their constituents, i.e. by distinctive features. And the breaking up of the phonemes into distinctive features follows precisely the same tested devices as the division of the morphemes into phonemes. 'By studying the possible *commutations*' we obtain, e.g., a French phonemic 'paradigm' /bu/ : /mu/ : /pu/ : /vu/ : /du/ : /gu/, and thus we find out that the phoneme *b* in /bu/ can be decomposed into five commutable elements: *b/m*, *b/p*, *b/v*, *b/d*, *b/g*. In examining the same phoneme in other environments we confirm

³ A. Martinet, *Phonology as Functional Phonetics* (London, 1949), p. 3.

⁴ A. Martinet, "Un ou deux phonèmes", *Acta Linguistica*, I (1939), pp. 94-103 (see p. 95).

⁵ L. Hjelmslev, *Omkring Sprogteoriens Grundlaeggelse* (København, 1943). = *Prolegomena to a Theory of Language*, transl. by F. J. Whitfield (Baltimore, 1953). For Hjelmslev (*Omkring* . . . , p. 66 = *Prolegomena* . . . , p. 46) "commutation" is the name of a correlation.

⁶ W. F. Twaddell, *On Defining the Phoneme* (Baltimore, 1935), p. 44.

⁷ Martinet, *Phonology* . . . , p. 4. Cf. R. Jakobson, "On the Identification of Phonemic Entities", *TCLCopenhagen*, V (1949), pp. 205-213 (see p. 212).

⁸ Twaddell, *On Defining* . . . , pp. 47-48.

this scheme. Cf. /bo/ : /mo/ : /po/ : /vo/ : /do/ : /go/, etc. Having ascertained a set of 'micro-features', as we could call them after the model of Twaddell's 'micro-phonemes', we may broach the question of the 'macro-features' and we easily detect a set of proportions, as for instance - /bu/ : /mu/ = /du/ : /nu/ ; /bu/ : /pu/ = /du/ : /tu/ [...]"⁹

In this description the devices are indeed precisely the same. When Jakobson analyzes a morpheme into phonemes, he sees that the two words /bu/ and /pu/ are formally identical except for the phonemes /b/ (in /bu/) and /p/ (in /pu/). When he analyzes a phoneme into features, he notices that the two words /bu/ and /pu/ are formally exactly the same except for the features voice (in /bu/) and voicelessness (in /pu/). But we cannot follow Jakobson any further: we are unable to see why these two analyses should be two successive steps, for both observations can be made simultaneously at the very beginning of the inquiry. Yet the passage quoted clearly implies that the latter analysis presupposes the completion of the former. Jakobson is entirely right when he thinks it necessary to "examine the same phoneme in other environments", in order to establish the "micro-features", but he does this already at a moment when he is not yet entitled to know that this phoneme is the same. For how does Jakobson identify the realizations of one phoneme in different positions? Here we run into a difficulty which Jakobson does not mention: the establishing of the "micro-feature" voice within the phoneme /b/ presupposes that the "macro-phoneme" /b/ has been established previously. But the "macro-phoneme" /b/, in turn, cannot be established before we are sure that the "micro-phoneme" /b/ in /bu/ differs from the "micro-phonemes" /p/ in /pu/, /m/ in /mu/, etc., in the same way as the "micro-phoneme" /b/ in /bo/ differs from the "micro-phonemes" /p/ in /po/, /m/ in /mo/, etc., in other words before we have established the "micro-features" voice, orality, etc.

We cannot but say that Jakobson's assumption of the same devices for the breaking up of the phonemes into distinctive

⁹ Jakobson, "On the Identification...", (p. 207).

features and the division of the morphemes into phonemes leads us into a deadlock. The only feasible solution seems to be that we give up the attempt to find the "micro-features" before the "macro-features". But then the distinction between "micro-features" and "macro-features" does not make much sense any more, we just have distinctive qualities. And indeed, there is not the slightest necessity to assume with Jakobson that the equation $/bu/ : /mu/ = /du/ : /nu/$ can be set up only after the equation $/bu/ : /mu/ = /bo/ : /mo/$ has been established. At the same stage of the analysis where we are able to make the one observation, we can make the other, too. And again I do not see why this should not be at the very start of the procedure.

The point of view of Martinet is less disputable: there is for him no parallelism between the two stages of phonemic analysis. His statements are diametrically opposed to Jakobson's.

In his London lectures¹⁰ Martinet says: "It is obvious, of course, that the analysis which enables us to divide phonemes into relevant features is of a different nature from that which has led us to isolating phonemes in a sequence. On the one hand we extricate elements from a synchronous whole through a phonetic observation of the motions of speech organs. On the other hand we content ourselves with acknowledging differences in acoustic perceptions: *-ake* it is, to our ears, identical in *make it* and in *bake it*; *b-* is, as a whole, different from *m-*." One look at the statement of Jakobson quoted above is sufficient to make it clear that what Martinet calls obvious is far from obvious to others.

One thing that strikes us as not reasonable in the theories of both Jakobson and Martinet is the different value attached to the use of phonetic (organo-genetic or acoustic) criteria, for the identification of phonemes on one hand, and for the identification of distinctive features on the other. In the first case both scholars consider phonetic resemblance an entirely insufficient criterion, but in the second case it plays in their theories the role of an absolutely decisive factor. When they arrive at the conclusion that the

¹⁰ Martinet, *Phonology* . . . , p. 3.

feature which distinguishes the *b* of *beak* from the *p* of *peak* is identical with the feature distinguishing the *b* of *cab* from the *p* of *cap*, what else do they bring forward to prove this but precisely the phonetic resemblance of the two distinctive features? But why should we, then, hesitate to establish the identity of the two *b*'s in *beak* and *cab* by means of their own phonetic resemblance, instead of attaining the same end in a roundabout way which does not yield additional criteria of a different nature?

To be sure, the cases where difference of opinion may exist are not so numerous in the identification of the distinctive features; but he who thinks that the solution is in all instances objectively certain makes a profound mistake. We can imagine very well that there are persons who do not want to accept the equation taken from a paper by Jakobson and Lotz¹¹: $l : r = z : d$; *l* and *z* being "continuous" as compared with the "intercepted" *r* and *d*.

Martinet writes: "In all those languages where [p] and [a] cannot stand in direct opposition to each other they have to be considered as belonging to different phonemes because their relevant features are different."¹² When he wrote this, he perhaps did not foresee that it is possible to identify relevant features in the following way: p (diluted and grave) : t (diluted and acute) : k (saturated) = f : s : $x = m$: n : $\eta = u$: i : a , as Jakobson and Lotz do (*o.c.*, pp. 152f.). We just have to imagine a language with these phonemes where the only admitted syllable type is consonant-vowel: in this language *u* will be accordingly the "archiphoneme" of *p*, *m* and *f*.¹³

A well-known case which allegedly shows the importance of the distinctive features for the identification of phonemes is the problem of German, Dutch and English η and *h*.¹⁴ They are said to represent two different phonemes, because their distinctive features are not identical. Actually, it all depends upon the way in

¹¹ R. Jakobson & J. Lotz, "Notes on the French Phonemic Pattern", *Word*, V (1949), pp. 151-158 ((see pp. 154f).

¹² Martinet, *Phonology*..., p. 19.

¹³ Compare Hockett, *Manual*..., p. 155.

¹⁴ Martinet, *Phonology*..., pp. 19f.

phonemes has changed into the problem of the identification of distinctive features, which is no easier to solve. Phonetic resemblance plays the same ambiguous role. I am even inclined to believe that a preconceived idea of the phonological difference of *h* and *ŋ* (based on the lack of phonetic resemblance) has tipped

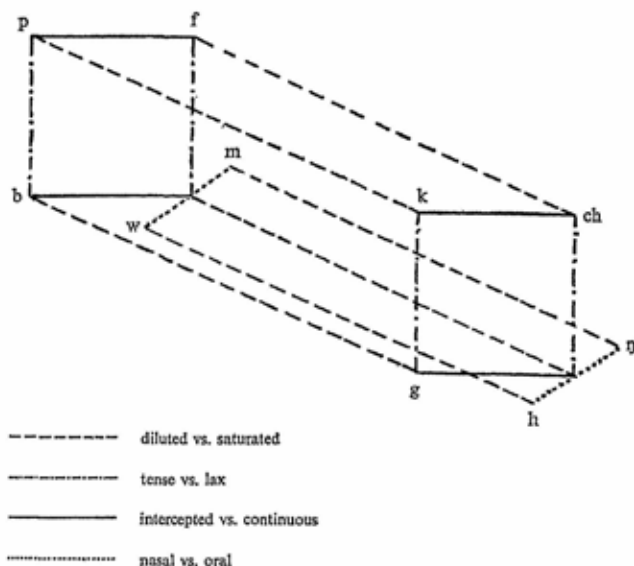


Fig. 2

the scale in favor of one of the two possible interpretations of the distinctive features and furthered the conclusion that "the two of them have hardly any phonological traits in common".¹⁶

I believe that we ought to subject the distinctive features to a test in order to prove their functional value. Our first task is to seek a method by which this can be done.

3. It seems advisable to approach the identification of phonologic entities using "the observable responses of members of the com-

¹⁶ Martinet, *Phonology* . . . , p. 20.

munity"¹⁷ as a criterion. This may go by way of substituting one element of sound in a given word for another: if native speakers recognize the resulting word as the same one departed from, the two elements of sound are functionally identical in this given environment. Theoretically, this is implied by the term "commutation", which means "substitution". But there is very little substitution in the procedure as it is usually applied. It is rather a comparison of two similar sound complexes in order to find out whether or not their meaning is the same.

The identification of phonemes in different environments by means of substitution is thought impossible. In this connection Martinet may be quoted: "But how do we know that the *m-* of *mail* and that of *mole* are identical? Is it scientifically satisfying to say that if we interchanged them we would still obtain the words *mail* and *mole*? How should we fare if we replaced the *b-* of *bad* by that of *cab*? In Danish, where all vowels are lowered when in contact with [r], if we replaced the vowel of *send* 'send' by that of *ret* 'right', we would no longer have *send*, but something that Danes would probably understand as *Sand* 'sand', though nobody will deny that *send* and *ret* have the same vocalic phoneme."¹⁸

Because the experiment of substitution fails to help us, Martinet thinks it necessary to have recourse to the distinctive features. It is – I repeat – a little surprising that he does not even mention the requirement of a proof based on substitution with regard to the identification of the distinctive features.

Yet I believe we ought to subject precisely the distinctive features to a substitution test, and if we do so we have the best chance to attain reliable results.

When we transplant a phoneme (or any additive entity) from one form to another (e.g. in Danish the vowel from the word *ret* to the word *send*), we necessarily remove its realization with all its functional as well as nonfunctional properties. It may happen, then, that a

¹⁷ Twaddell, *On Defining* . . . , p. 37.

¹⁸ Martinet, *Phonology* . . . , p. 4. See also A. Martinet, "Au sujet des 'Fondements de la théorie linguistique' de Louis Hjelmslev", *BSL*, XLII (1942-45) pp. 19-42 (pp. 37-39).

feature which in the first form was not distinctive will become so in the second form, and vice versa. The same situation appears when we consider distinctive features additive entities and transplant a degree of openness: what in the first form was functionally open may in the second be functionally closed. Obviously, such an operation makes no sense, for it does not make the relationship between two *e*'s in different environments any clearer.

But when we start from the assumption that the distinctive features are qualities, the situation changes. A quality is present only when its opposite may occur in the same environment, or as Jakobson formulates it: "The phoneme itself is not a term of opposition, e.g. the phoneme *b* does not call univocally, reversibly and necessarily for a definite opposite, while any distinctive feature does. There is no possible distinctive tenseness without laxity, [...] no presence without absence of nasal resonance, and *vice versa*."¹⁹

All sounds which can be produced by the human speech organs form together a multidimensional continuum. Every language delimits within this continuum a number of multidimensional areas. Since a language functions as a code, these areas are discrete. Each of them is occupied by a phoneme. The phoneme covers with its admissible allophones the entire area. Because of the multidimensional character of a phoneme-space, we cannot study it at once in its totality: we investigate the dimensions separately. Every dimension is again a continuum, which is dissected by the phoneme borders into discrete portions.²⁰

A dimension may be represented as passing by gradual change from one extreme point to the other; it may be referred to by indicating the two extremes, such as in articulatory terminology "high - low", "no stricture - occlusion", "back - front", "oral - nasal". The discrete parts into which a dimension is divided by the rules of the code, are the distinctive qualities of the language. (It goes without saying that for different phonemes the dissection of

¹⁹ Jakobson, "On the Identification . . .", p. 208.

²⁰ Compare *Psycholinguistics* . . . , p. 78.

identical dimensions may be different as to the number and the place of the relevant borderlines).

This assumption enables us to perform an operational procedure to which the term "commutation" is applicable in its full sense. As a point of departure we take, for example, the same Danish word *send*. We want to study the functional qualities of the sound *e*. So, if we think that it might be high, we make it lower and see if we get a different word. If we suspect it of being low, we make it higher and observe what happens; and in the same way with the other possible distinctive features.

This approach has the advantage that it is not based on subjective comparison of two distinctive differences of sound, e.g. the differences between French *t* and *s* on one hand, and *r* and *l* on the other. If we think that these two differences are identical and we consider the distinctive features entities, we can assume that these functional entities are differently realized in different contexts: in the given instance we may recognize the same feature "intercepted" in both *t* and *r*, because in *t* it is in contact with the feature consonant, in *r* with the complex feature "consonant/vowel". This is what Jakobson and Lotz propose to do. They can make the identity more or less probable referring to the phonetic resemblance, but in the end they cannot prove it, for the resemblance will always depend upon a subjective interpretation, exactly like the similarity of the phonemes *b* in *cab* and *beak*. At this point, many functionalists do the same thing they reproach Jones with. It is remarkable how similar Jakobson's approach to the distinctive feature is to Jones' definition of a phoneme. In both cases there appears a family of phonetically related, mutually exclusive members. Jones: "A phoneme is a family of sounds in a given language which are related in character and are used in such a way that no one member ever occurs in a word in the same phonetic context as any other member."²¹ Jakobson: "If certain phonemic distinctions possess a common denominator and are never observed to co-exist within one language, then they may be interpreted as mere variants of a

²¹ Jones, *The Phoneme*..., § 31, p. 10.

single opposition."²² What is identical in the definitions is the procedure of identification. Jones himself adds that "the restriction 'related in character' is of necessity vague", but this is only a practical inconvenience. I consider it a more serious disadvantage that this condition is of a purely physical nature. When Jakobson refers to the "common denominator" of phonemic distinctions, he thinks that the vagueness (inherent in Jones' "resemblance") is to a large extent eliminated, "owing to the amazing advance of modern acoustics", but my objection that the functional aspect is disregarded remains unaltered.

But if we assume that the distinctive features are qualities, we observe the changes in the degree of closure when we go from *s* to *t*, and we can repeat these changes taking as our starting point *l*. Our conclusion depends, then, on the interpretation of the obtained result by a member of the French community.

We start here from articulatory facts, but we may prefer acoustic data instead. Only it is not very probable that these will lead to different results, though we must reckon with this possibility, of course.²³ The conviction of some scholars that the articulatory facts furnish sufficient data for discovering the relevant phonetic properties is fortified by the hypothesis, recently winning ground, that "s'il existe un 'invariant' qui permette de distinguer un lieu d'articulation consonantique d'un autre, il est plutôt dans le geste articulatoire que dans le trait acoustique: la forme acoustique de la parole serait perçue, non directement, mais indirectement par référence au geste articulatoire qui est le même pour plusieurs valeurs acoustiques différentes".²⁴ Reference to the kinesthetic sense may explain why certain consonants are perceived as different although they are represented by identical segments of the acoustic spectrum: "dans un cas tout spécialement frappant, une même

²² Jakobson, Fant & Halle, *Preliminaries* . . . , p. 7.

²³ Jakobson & Halle, *Fundamentals* . . . , pp. 33ff.

²⁴ P. Delattre, "Les indices acoustiques de la parole. Premier rapport", *Phonetica*, II (1958), pp. 108-118 & 210-251 (see p. 228). Compare also E. Fischer-Jørgensen, "What can the New Techniques of Acoustic Phonetics contribute to Linguistics?", *Proceedings of the Eight International Congress of Linguists* (Oslo, 1958), p. 433-478 (pp. 466-469).

explosion d'une fréquence de 1440 cps, est entendu comme /p/ quand elle est unie à [i] et comme /k/ quand elle est unie à [a]" (ib.).

All distinctive features are terms of oppositions. Theoretically it is very well possible that besides the extreme terms also intermediate terms are relevant. It is wise not to overlook this possibility as long as Jakobson's theory of the binary character of the phonemic oppositions has not outgrown the speculative stage.

Our starting point is the supposition that certain oppositions exist in a given language. We take any word of this language and let its sounds (not bothering whether they are simple or complex) move along one dimension in both directions. Several things may happen. Either the artificial word we obtain is not recognizable as a possible form of the language. Or we get a word which native speakers will interpret as a different, though not necessarily occurring, form. In the former case we have come outside the field of language sounds and find data for the distinction of speech sounds from nonspeech sounds in the given language, but not for the setting apart of the phonemes from each other. In the latter case we have discovered a relevant opposition. It may also happen that before reaching a distinctly different form we first get a sound complex which is uninterpretable because it may be as well the first as the second form and does not occur in normal speech.

Usually the words we obtain in this artificial way are not exactly identical with the normal pronunciation of the forms recognized in it. It is to be expected that if we follow back along the same dimension, starting from the normal realization, we may not arrive at the form originally started from, but at a third form. In this case we must study the relationship between the first and the third form.

Examples: in Dutch (and no doubt in many other languages) /p/, when nasalized, results in a voiceless sound which will be interpreted as /m/, although /m/ is normally voiced. Going back from a normal /m/ to an oral sound, we get /b/. The voice-correlation turns out to have no function with /m/. More complicated is the following case. The Dutch vowels /ö/, /e/ and /o/, as in /stön/ *steun* "support",

/sten/ *steen* "stone" and /stof/ *stoof* "foot-warmer", pronounced with an increasing degree of openness, will finally yield different sounds which are all interpreted as /a/, as in /stan/ *staan* "stand" or /staf/ *staaf* "bar". The features mutually differentiating these sounds (front + rounded, front + unrounded, back + rounded, respectively) are irrelevant for the open vowel /a/. When we pronounce an otherwise normal /stan/ with the degree of openness of the vowel characteristic of /ö/, /e/, /o/, we get a hardly recognizable word. The unrounded back vowel of this word belongs to a part of the sound continuum which is not occupied by a Dutch phoneme.

In many instances it is possible to check the relevance of a feature found in the manner described above by suppressing it and trying to pronounce the same linguistic form with the opposite feature. For instance, we have a form /ta/, we suppose that the /t/ is voiceless, we make it voiced and obtain [da], which is indeed a different form. But now we try to pronounce the form /da/ with a distinctly voiceless consonant (this can be done only by a native speaker). If the attempt is successful, the opposition /t : d/ is not voiceless : voiced, but something else, e.g. fortis : lenis.

I do not recognize, however, that a functionally simple feature, a feature which, in other words, constitutes a minimal (or "direct") opposition between two phonemes, is also necessarily physically and physiologically simple. It is difficult to see why a phonemic property should always be limited to one dimension of the sound continuum. It is not excluded a priori that /t/ and /d/ in some languages are distinguished from each other as voiceless + fortis and voiced + lenis. There seems to be nothing against describing them so. In a language where [i], [ʊ] and [ü] are all interpreted as one phoneme /i/, and [u] as /u/, the latter can be defined as back + rounded. Most modern linguists take the opposite stand, albeit tacitly: they select one of the two qualities, either back or rounded, as the decisive one and regard the other as redundant. I consider this a dubious axiom. An exception is De Groot, who writes: "As far as I can see it is assumed that wherever two of the twelve oppositions are combined, e.g. tense with unvoiced, or/and lax with voiced, one is autonomous, and the other concomitant."

It may be asked perhaps whether both cannot be autonomous at the same time, so that the term 'complex' feature or 'composite feature' might be applied."²⁵

This is also the opinion of Jones: "If two sequences are distinguished by simultaneous changes in two attributes, the distinction is a minimal one when the amount of change in each attribute is not of itself sufficient to differentiate words in the language in question."²⁶ But instead of referring to "the most usual pronunciation", as Jones does when he concludes that the vowel of *deed* differs from that of *did* "both in length and in quality", I would prefer first to examine by means of an experiment whether one of these features is indeed not of itself sufficient for this purpose: the conclusion will then depend on the interpretation of [dɪ:d] and [did]. If neither of these artificial forms contains enough indications for a recognition, the two properties constitute together indeed a minimal distinction. A choice of one of the equivalent features as relevant and the other as redundant is in such cases arbitrary and, therefore, senseless.

For the given examples we supposed that the two features were always applied simultaneously, but it may also happen that one phoneme is characterized by the presence of two features and the other by the opposite of either of them. For instance, Dutch /stəm/ *stom* "mute", spoken with widened vowel, gives /stam/ *stam* "stem", but produced with the same degree of closure as /stəm/, but without rounding of the lips, it yields the same form /stam/. The two pronunciations differ of course, but this is phonemically of no importance. Thus /ə/ must be both rounded and mid; otherwise it will be confused with /a/. But /a/ may be either unrounded + low, or rounded + low, or unrounded + mid. The relationships are shown in Figure 3. This formulation may be shortened into: /a/ is either unrounded or low; but it would be a wrong simplification to retain in the description only one of these features and to say: /a : ə/ = unrounded : rounded; or /a : ə ,

²⁵ A. W. de Groot, Review of Jakobson, Fant & Halle, *Preliminaries*..., *Word*, IX (1953), p. 62.

²⁶ Jones, *The Phoneme*..., p. 16.

$\varepsilon, \Lambda / = \text{low} : \text{mid}$. (The latter equation would be fully acceptable in the case of the tense vowels $/a : o, e, \ddot{o} / = \text{low} : \text{mid}$; see above, pp. 28-29.)

It does not matter from how many supposed oppositions we start the analysis, provided that we are able to obtain every possible form in the language while starting from any word and doing nothing but changing them repeatedly along the lines of the dimensions and, of course, adding and subtracting sounds.

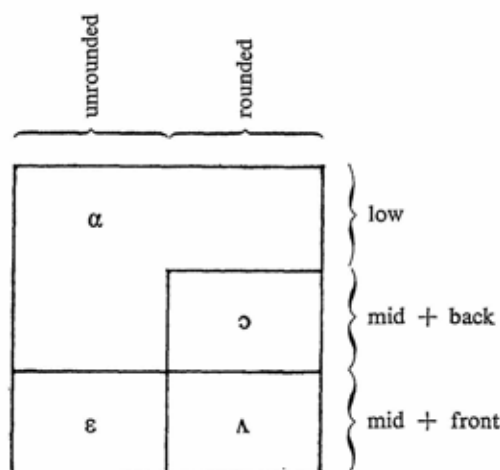


Fig. 3

4. The procedure outlined here has to encounter the difficulty that the continuum of speech sounds is said to fall into two disparate sections: the consonants and the vowels. The separation is not considered so absolute, however, that it prevents the existence of phonemes which have, through their allophones, a place among both groups, such as $[i/j]$, $[u/w]$. Martinet speaks of "two different paradigms with contacts limited to the domain of 'sonants' (liquids, nasals and semi-vowels), phonemes which, in certain languages, will 'commute' equally well with vowels and consonants".²⁷ In

²⁷ A. Martinet, "Phonetics and Linguistic Evolution", *Manual of Phonetics*, edited by L. Kaiser (Amsterdam, 1957), pp. 252-273 (p. 264).

languages where *i* and *j* are distinct phonemes, their only difference is often described as vowel versus consonant. If this is right, there must be a zone of transition between the continuum of the vowels and that of the consonants. Consequently, a word like *aloud* can, through a continuous shifting of its initial segment along various dimensions, in the end be changed into *cloud*. In this way the phonemes /a/ and /k/ are, with all their differential qualities, incorporated in one system.

This incorporation has been accomplished in a completely different way by Jakobson, who, in his chart of the Serbocroatian phonemic system,²⁸ presents the phonemes *a* and *č* as forming a direct opposition, *a* being marked by the presence of the feature "vocality" and *č* by its absence, while they have the other features in common. This gives a somewhat simplified impression of Jakobson's conceptions, for in this chart he did not yet make a distinction between absence of a feature as opposed to its presence on the one hand, and absence of a feature on account of the lack of a phoneme carrying the feature (so to say: by neutralization) on the other. So, in both *č* and *a* the feature "voicing" is said to be absent, but *č* is by this absence distinguished from *dj*, whereas *a* simply does not participate in the opposition. Nevertheless, the gap between vowels and consonants is bridged.

In *Preliminaries* and in *Fundamentals* the opposition vowel: consonant is split up into two separate oppositions, termed vocalic: nonvocalic and consonantal: nonconsonantal, but the opposition *i*: *j* is assigned to neither of them: "The French syllabic [i] and nonsyllabic [j] are phonemically opposed to each other as tense and lax."²⁹ Consequently, there are no pairs differentiated solely by the opposition vowel: consonant. Moreover, on account of the use of minus signs in the charts, the reader can no longer infer from the proposed systems a statement like "*a* is the vocalic counterpart of *č*", as he might do when studying the analytic description of Serbocroatian. In the later publications, the only

²⁸ Jakobson, "On the Identification...", p. 209.

²⁹ Jakobson, Fant & Halle, *Preliminaries*..., p. 46.

fact connecting the vowels and the consonants is the occurrence of the same oppositions in both parts of the system: compact : diffuse (e.g. $e : i = k : p$ or t), grave : acute (e.g. $u : i = f : s$), flat : plain (e.g. $u : u = k^w : k$), tense : lax (e.g. $p : b = o$ in French *saute* : ə in *sotte*), nasal : oral (e.g. $n : d = \tilde{a} : a$).

When we compare the way Jakobson surmounts the vowel-consonant antithesis with the more traditional view, which finds a transition in the area occupied by i/j , u/w , we see that Jakobson maintains a part of it: he puts the labialization of a consonant on a par with the u -color of the vowels. On the other hand, he diverges from it in that he separates the palatalization of the consonants from the i -color of the vowels. The Russian opposition $b' : b$ is called sharp : plain, whereas $i : u$ (e.g. in Turkish) is acute : grave, like $s : f$, $t : p$, etc. A more traditional view has been advocated by Martinet; we can find it in a report, unfortunately brief, of a gathering of the Société de Linguistique de Paris: "Il n'y a pas, phonologiquement, de limite nette entre les phonèmes ouverts et fermés; la distinction de consonnes et voyelles est seulement commode, mais arbitraire. M. Martinet montre comment, en considérant le degré progressif d'ouverture, les phonèmes se situent, structurellement, sur des plans différents; il en distingue cinq:

I	{	p	k'	k ⁰
	{	b	g'	g ⁰
II	{	φ	ç	w
	{	β	j	w ⁰
III			i	u
IV			e	o
V	{		ε	ə
	{		a	etc." ³⁰

The distinction of vowels and consonants causes serious trouble to the phonetician, too. The criterion is "cavity friction" versus

³⁰ Séance du 17 juin 1939 de la Société de Linguistique, *BSL*, XL (1938), pp. xxv-xxvi.

"local friction".³¹ Nevertheless, high vowels are often pronounced with clearly audible local friction. A phonologist has the advantage of working with relative criteria. In other words, if the [i] spoken with local friction is not relevantly different from an otherwise identical sound with no such friction (or with less of it), it is phonemically characterized by "absence of local friction". In many languages there are phonemes /ə/ and /h/, both distinguished from other phonemes by zero local friction and zero cavity friction in the mouth. The phonemes with relevant local friction are distinguished from each other by the degree, place and shape of the closure responsible for the friction, and likewise the phonemes with relevant cavity friction. It does not seem advisable to call the phonemes with relevant local friction in the mouth "consonants", for then /h/ would not be a consonant, and neither would /n/, /l/ and /w/. The terms "consonant" and "vowel" are better reserved for distribution classes of phonemes.

It follows that there is no dimension running from vowel to consonant, but rather two disparate dimensions, as is rightly pointed out by Jakobson (see above). Consequently, there is no zone of transition, either. At best we might regard as such the area of the continuum occupied by the phonemes which have both type of friction or which lack both of them. However, not the liquids are characterized by both frictions (although Jakobson calls them "vocalic and consonantal"), but rather the palatalized and labialized consonants. In this respect, Martinet's view is no doubt correct.

If there is no zone of transition in the strict sense of the word, what then is the position of the so-called "semi vowels" in languages where the oppositions *i : j* and *u : w* are relevant? Let us study these sounds in a concrete language. In Dutch there is an opposition between /ia/ and /ja/. At first glance absence versus presence of local friction seems to account for this difference. If we pronounce the group [ia] with local friction in the first element, the result is

³¹ K. L. Pike, *Phonetics. A Critical Analysis of Phonetic Theory and a Technic for the Practical Description of Sounds* (Ann Arbor, 1958), pp. 71, 138ff.

indeed interpreted as /ja/ (unless this first element was in the original group prominent as to pitch or stress – in that case the result is scarcely interpretable). However, the outcome is the same when we pronounce [i] without local friction, but voiceless. And both voicelessness and friction are dispensable when we deprive [i] of the chest pulse with which it is realized in the group /ia/. So the /j/ is phonetically distinct from /i/ as either nonsyllabic or voiceless or fricative. The distinguishing feature is phonetically complex, but phonemically simple. The opposition *i* : *j* may be termed “sonorous : nonsonorous”; it recurs in *u* : *w* and *a* : *h*. The phoneme /h/ has two variants, voiceless and voiced; voiceless [h] is nonsyllabic and thus less sonorous than voiceless [ə], voiced [h] is marked by friction and consequently also less sonorous than voiced [ə].

The dimensions “with oral local friction or obstructed – nonobstructed” and “with oral cavity friction or resonated – nonresonated” belong to a larger series of disparate dimensions. The other dimensions characterize the passageway of the pulmonic air as “lengthwise divided or lateral – nonlateral”, “breadthwise divided or flapping – not flapping”, “nasal – nonnasal” and “oral plus nasal, or nasalized – nonnasalized”. The negative terms of these oppositions are all combinable into one phoneme, such as /ə/ or /h/; this means that for these phonemes the air, after leaving the larynx, egresses further undifferentiated through the undivided mouth. Some of the positive terms are mutually exclusive, like nasal and nasalized, or nasal and obstructive; others are combinable, such as resonated and nasal in the palatalized apical nasal [n'], resonated and nasalized in [ã]. Other dimensions, such as “high – low” or “sonorous – nonsonorous”, presuppose these fundamental dimensions.

When the positive terms of two disparate dimensions X and Y can be present in one and the same phoneme, no phoneme carrying only positive X forms a direct opposition with a phoneme carrying only positive Y. For example, the Russian nonresonated obstruents are not differentiated by a direct opposition from the resonated nonobstruents, because there are also resonated obstruents. When, however, the combination of the two positive terms does

not occur, such phonemes may be considered to constitute a direct opposition, no matter whether this nonoccurrence is universal, as in /b : m/, or conditioned by the system of a given language, as obstrusive versus resonated in English.

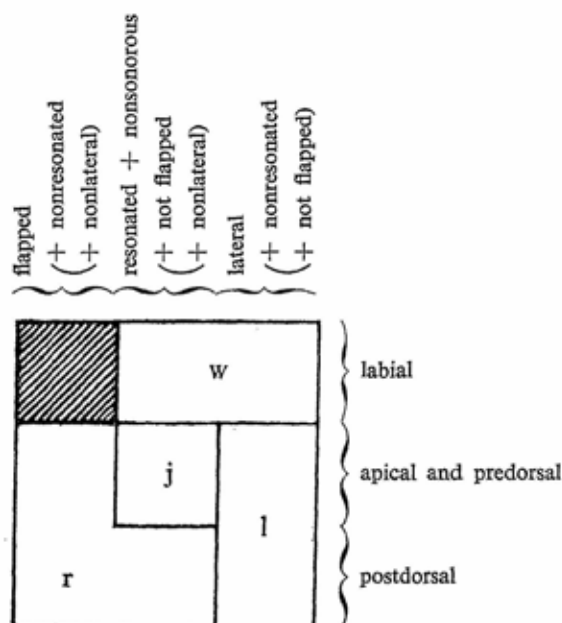


Fig. 4

Even when dimensions are disparate, it sometimes happens that an area occupied by one phoneme is located partly in one and partly in another of these dimensions. This is the case for the Dutch phoneme /r/, as is shown in Figure 4. Among the variants of /r/ we find not only flapped nonresonated sounds, but also not flapped resonated sounds. There are at least three different realizations of this phoneme, occurring in the same positions in the word, and all of them equally common: a lingual trill, a uvular trill and a kind of semi-vowel. The last sound may be produced in the following way. If we start from the word /haj/ *haai* "shark" and

move the articulation place of the final segment backward, we get /har/ *haar* "hair". The sounds are difficult to distinguish for a foreigner, even when he has got a phonetic training. In textbooks of phonetics this realization of the /r/ is usually disregarded, probably it is considered the result of a degeneration. Phonemicists also take the trill as the distinctive feature, although /r/ functions perfectly without it. But this is an oversimplification: the phoneme /r/ is differentiated from /j/ either by its place of articulation (a one-dimensional difference) or by absence of resonance and flap articulation (a two-dimensional difference). The place of /r/ in the system can not be satisfactorily described as long as we cling to the current theory of phonemic oppositions, which inevitably forces the investigator to confront /r/ and /l/ and to neglect the realization of /r/ as a nonsonorous not flapped continuant, or – if this realization is not disregarded – to interpret it as phonemically flapped or phonemically discontinuous, so that the alleged phonemic property is contrary to the phonetic character of one of the common realizations. Only the idea of a parcelled-out continuum of sound enables us to account for the functional unity of two so different speech sounds.

It is an inconvenience of the method proposed here that, instead of neat formulations like "the phoneme X consists of the relevant features $a + b$ ", we now often get a more intricate statement, such as "X consists of $a + b$ or of $c + d$ ". X may be distinguished from Y, marked by $e + f$. But in some environments we may find an allophone X', only marked by $a + b$, and on the other hand Y', marked by $c + d$ or by $e + f$. When ascertaining the "macro-phonemes", we have a right, of course, to identify X and X' as well as Y and Y', although the micro-phonemes have not all their features in common. This inconvenience, however, does not counterbalance the advantage of a phonemic analysis resting on more solid and objective grounds.

5. Against this method of phonemic analysis it may be objected that it relies too much on the judgment of native speakers. It depends entirely on their decision whether two sound complexes are considered realizations of one form or of two forms.

Usually linguists refer in this connection to meaning. But this cannot be done consistently. An example may illustrate this: the Russian words *škaf* and *škap* are usually regarded as synonyms.³² If we base, however, our criterion for the distinction between relevant and nonrelevant differences of sound upon meaning, we must admit that in Russian immediately after *ška-* the opposition stop versus continuant is neutralized in contact with labiality and orality. Nevertheless, nobody seems to accept this formulation. To every Russian [*škaf*] and [*škap*] are no doubt two different forms. This criterion should prevail everywhere as it does in this case.

I take an English example from Hockett³³: *root* may be pronounced [*ruwt*] or [*rut*]. In such cases the environment, *ška-* or *r-t*, is usually considered too complicated or too specific for the assumption of a neutralizing influence, but this can only mean that probably a native speaker will not be inclined to interpret the forms as the same: we can in no way dispense with an appeal to his interpretation.

I agree with Jones: "The sounds of separate phonemes do not necessarily distinguish words, but they are capable of doing so, and generally do so."³⁴ Like Jones, I prefer to identify the phonemes without referring to their semantic function. But when I start from the set of rules stored in the decoding mechanism, I do not deny that these rules originate exactly under the influence of the semantic function of the phonemes.

Another objection may concern the fact that artificial forms are used to throw light upon the analysis of existing forms. Of course, the artificial forms must be presented to the informant not as mutilated words of his language, as is done in recognition tests, but as forms produced under optimal conditions. I admit that this requires a certain preparation of the informant, or, as Hockett formulates it: "The basic problem in phonologic field work is to bring it about that there reside, in a single nervous system, both

³² *Tolkovyj slovar' russkogo jazyka*, pod red. D. N. Ušakova, IV, p. 1350.

³³ Hockett, *Manual*..., p. 146.

³⁴ Jones, *The Phoneme*..., p. 14.

the native or native-like active control of the phonologic system, and the clear understanding of what is meant by 'sounding same' and 'sounding different'." ³⁵ We should take pains to avoid "complaisant listening" (as Hockett calls it, p. 201) on the side of the informant. But once the informant knows what is asked from him, he will be able to distinguish two ways of interpretation, according as he takes the one or the other stand. When we hear, for example, *tem mins sem* "ten minutes to seven", ³⁶ we may first replace this form observed in very rapid style by its ordinary pronunciation, and thereupon analyze this latter into phonemes. But then the first part of the procedure is not of a linguistic nature, though often performed in everyday practice. A well-prepared informant, however, will be able to indicate which distinctive features are still present in the slipshod realization of the words, so that there will be also phonemically two or more forms, one normal and the other(s) slovenly. This approach helps to bridge the gap between phonetic and phonemic investigation. I have often heard it said by adversaries of phonemics that many so-called distinctive features can be omitted and the forms are nevertheless perfectly understood. Allegedly, this is a death-blow to all functionalistic thinking. And indeed, it cannot be denied that there is a good deal of truth in such a reasoning, unless we admit that a pronunciation which lacks some of the expected distinctive traits is in reality a different *form*, which stands to the more "complete" form in a stylistic relationship of deviating versus normal (not in a statistical sense).

From a psychological standpoint, the recognition of the word *apple* is achieved in much the same way as the recognition of the object APPLE. ³⁷ It is obvious that the object APPLE is not characterized by a permanent set of distinctive qualities. Neither is, as a consequence, the word *apple*, but only as far as recognition is concerned. Hockett gives the example: *please pass the piscuits*, the last word of which may be understood as *biscuits*. This inter-

³⁵ Hockett, *Manual* . . . , p. 145.

³⁶ So written by Jones, *The Phoneme* . . . , p. 228.

³⁷ *Psycholinguistics* . . . , p. 56.

pretation would be right in terms of the speaker's purpose, but wrong in terms of phonemes.³⁸ In spite of the superfluity of the initial voicing for a recognition, the word *biscuits*, linguistically spoken, carries this quality, and, again from a linguistic point of view, this word *biscuits* does not occur in Hockett's example. If we did not take this stand, we could not establish the phonemic difference of the forms /hæv, həv, əv, v/ in English,³⁹ for [v] might be interpreted as a realization of /hæv/.

The attempts to combine the psychological and the linguistic approaches have led to the introduction into linguistics of such nonlinguistic units as syllables and other "vocalic skill components".⁴⁰

Theoretically, there is nothing against the use of artificial forms. Moreover, in a certain way, they are rather frequently taken into consideration – albeit tacitly – for the possibility of substitution by real forms is in practice too restricted to satisfy the needs of the phonologists. There is a justification for doing so: many scholars think it can be decided without invoking the help of an informant whether or not a given artificial form is possible in a given language, whether or not its nonoccurrence is fortuitous.

I quote Hintze: "Und auch die Wortstruktur berücksichtigt Martinet (wenn er es auch nicht ausdrücklich sagt) dort, wo er als Argument anführt, dass die Vertauschung von [ž] in engl. [ledžə] mit Null möglich sei, wenn es auch zufällig kein solches Wort gebe. Denn woher weiss Martinet, dass *[ledə] eine im Englischen mögliche Wortform ist, wenn nicht eben durch die implizite Berücksichtigung der englischen Wortstruktur, die ihrerseits die Berücksichtigung der Silbenstruktur impliziert." ⁴¹

Hintze knows from the rules of syllable structure in English that [d] is possible after [le-] and before [-ə], though it does not

³⁸ Hockett, *Manual*..., p. 219; compare also p. 214.

³⁹ See E. A. Nida, "The Identification of Morphemes", *Language*, XXIV (1948), pp. 414-441 (pp. 432f.).

⁴⁰ *Psycholinguistics*..., p. 57; compare also *ib.*, section 3.2.

⁴¹ F. Hintze, "Zur Frage der monophonematischen Wertung", *Studia Linguistica*, IV (1950), pp. 14-24 (p. 17).

occur in this position. He knows that [d] occurs in combination with other sounds which are phonetically not too different from [le-ə] to make the occurrence of [d] in this position impossible. We note that this observation is of a purely phonetic nature.

Another example: I do not think that Hintze regards initial [tl-] as possible according to the English syllable structure, though [pl-, kl-] are frequent. Why does he not derive a rule that the English syllable structure admits initially a voiceless stop plus [l], e.g. *play*, *clay*? Probably the phonetic difference is too large between [p, k] on one hand and [t] on the other.

I am willing to assume that a phonetic law can be formulated which will enable us to decide in every single case whether the nonoccurrence of a combination is fortuitous or due to the rules of syllable structure. Long before Hintze, Twaddell concluded that "the absence of a form *[fet] is a fortuitous [...] characteristic of the [f-t] series".⁴² I admit that, working with this criterion, we are able to increase the number of data about a language by nonexistent, but possible forms. So we have in English side by side [ledʒə] and *[ledə]. But who tells us, then, that these two forms correspond to two different meanings? It is a similar question as with the Russian synonyms *škaf* and *skap* (see above); only in this latter case we are sure that we have to do with closely related meanings, in the former case it is only a possibility.

Twaddell and Hintze think that the criterion of syllable structure solves this problem in the sense that a possible, but by chance not existing form is always functionally different from all existing forms of the language. Then, what do they else than draw a conclusion about the probable judgment of native speakers, who will acknowledge *[ledə] as a possible form different from [ledʒə]?

I do not think it necessary to assume that fortuitous absence implies phonemic distinctness, but I reject a fortiori the idea that the fundamental absence of a form is a proof of its phonemic nondistinctness. At least, this is the way I understand Twaddell when he writes: "There is a phonological difference between the

⁴² Twaddell, *On Defining...*, pp. 50f.

forms *pill* and *bill*; there is no similar phonological difference between the forms *spill* and **sbill*." In my opinion, if English speakers interpret **sbill* as a different (though unknown) word, the voicelessness of *p* is just as indispensable for the characterization of *spill* as it is for *pill*.

When we study the English phonemes we find initially before [l-] only two voiceless stops, [p] and [k]. We cannot identify the [p] in this position on the basis of a permanent set of distinctive features, unless we start from at least one artificial form, for we need a correlation like *pail* : *tail* = *plot* : **tlot*. Only a native speaker can decide that the form **tlot*, considered as an English word, is different from the form *plot*, even if it has no meaning. In this way we know that the sound [p] is not a realization of an archiphoneme, product of the neutralization of /p/ and /t/ in this environment.

I am ready to admit that the absence of **[ledə]* in English is fortuitous, the absence of **[tlot]*, however, nonfortuitous, whatever this distinction may imply. Probably, an observable consequence is that **[tlot]* will be more easily misunderstood as *[klot]* or *[plot]*, but this difficulty can be surmounted by a careful preparation of the situation for the experiment.

On the other hand, the use of artificial forms presents a certain advantage. When we compare two functionally different sounds in realizations in which they occur in the language, how do we know that the phonetic difference we notice is precisely the one that functions? Studying *r* and *l* in French, Jakobson and Lotz were, of course, right that *r* has more of a stop than *l*, but unless they prove experimentally that no realization of *r* where the stop character is absent can be unambiguously interpreted as *r* by a native speaker, how do they know that this is the pertinent difference? In their method of analysis the phonetic examination of a distinction comes *after* the establishment of its relevance. I have proposed in the present study a procedure which leaves the last word to criteria of function. Thus I agree with Mol and Uhlenbeck, when they say: "It is rather striking that linguists have rarely

executed any identification experiments",⁴³ but I do not share another opinion of theirs: "The problem which arises after carrying out the identification experiment is the problem of describing possible distinctive features" (p. 171), for in any such test the distinctive features are necessarily the input, about which the investigator has complete knowledge from the start: he himself changes it as he pleases.

Phonetic science should be applied not only after we have found the inventory of the phonemes, in order to describe their usual realizations and to arrange them into a system, but especially before setting up the list of phonemes: the study of pertinent borderlines and no man's lands between sounds is then the first step in phonemic analysis.

Finally, we get rid of a disadvantage of some practical importance, which is a consequence of the fact that it is not always easy to tell whether or not a given form should be regarded as belonging to a language. For the study of "syllable structure", as Hintze wants it, we need the whole material of the language, and this is to a large degree fluctuating.

Therefore phonemic analysis is mostly more successful in cases where students have at their disposal only a restricted number of forms of the language under study. In other words, the results of phonemic analysis are often inversely proportional to the investigator's knowledge of the language. Consequently, well-known languages, like French, English, German, happen to be full of intricate problems, whereas less studied languages lend themselves admirably to phonemic analysis. What Hintze⁴⁴ is unable to show on German, because this language, as he says, has too complicated a syllable structure, he can illustrate more easily on examples from the languages Kpelle, Ful and Sahidic-Coptic. Pike remarks: "In phonemic theory today considerable uncertainty remains", but after having mentioned some cases, he adds: "Although these

⁴³ H. Mol and E. M. Uhlenbeck, "The Analysis of the Phoneme in Distinctive Features and the Process of Hearing", *Lingua*, IV (1954-55), pp. 167-193 (p. 170).

⁴⁴ Hintze, "Zur Frage . . .", pp. 20f.

difficulties are severe, it is fortunate that one seldom finds them as strikingly so as in English.”⁴⁵ As to myself, I am inclined to concede to Dutch the honor of putting up the most stubborn resistance to current phonemic analysis, but I am convinced that when a Tübatulabal indian masters the technique, he will soon notice that it works perfectly for all languages except Tübatulabal.

When all kinds of forms, existing as well as artificial ones, are subjected to an interpretation test like the one described above, I do not see why anybody should not be able to set up the phoneme inventory of his own language, no matter how intricate its system is. In this study I am often forced to adduce Dutch examples, precisely because I do not find the same complications in other languages, although I am convinced that they can furnish analogous examples.

6. At this point we take up again the criterion of discreteness of linguistic entities. Applied to the procedure designed for the investigation of the linguistic role of a given phonetic dimension in a given environment, it demands that the number of segments delimited in this way be objectively demonstrable (in practice mostly two, seldom more than three). Whenever the number of segments can be chosen at random, the dividing is not supported by the code. This does not imply that all these arbitrary segments may not supply different information. In Dutch, for example, we can set up a whole range of places of articulation for the vowel in a word like *kaas* “cheese”, running from [æ:] to [ɑ:], and all of them definitely indicate a different degree of education or social status, but since they are not encoded, their number may vary from two to infinite and is restricted only by the perceptual abilities of the listener. The same applies to the duration of the *s* in French *insupportable*; its lengthening “gives information about the mood of the speaker, or at least that mood which hearers must suppose to be his at that particular moment”.⁴⁶

The idea has been most clearly formulated by Sapir: “To the naïve speaker and hearer, sounds (i.e. phonemes) do not differ as

⁴⁵ Pike, *Phonemics* . . . , pp. 63f.

⁴⁶ Martinet, *Phonology* . . . , p. 8.

five-inch or six-inch entities differ, but as clubs and poles differ. If the phonetician discovers in the flow of actual speech something that is neither 'club' nor 'pole', he, as phonetician, has the right to set up a 'halfway between club and pole' entity. Functionally, however, such an entity is a fiction, and the naïve speaker or hearer is not only driven by its relational behavior to classify it as a 'club' or a 'pole', but actually hears and feels it to be such."⁴⁷ And, in an other place: "Each member of this system is not only characterized by a distinctive and slightly variable articulation and a corresponding acoustic image, but also – *and this is crucial* – by a psychological aloofness from all the other members of the system. The relational gaps between the sounds of a language are just as necessary to the psychological definition of these sounds as the articulations and acoustic images which are customarily used to define them" (p. 35).

A sound dimension running from A to Z, when parcelled out phonemically, consists of three segments (at least): A–P allotted to a phoneme, a no man's land P–Q, and Q–Z allotted to an other phoneme. The two extreme segments carry a function. The function of a point is wholly defined by the mere fact that it belongs to one of the segments. Its place within the segment plays no role. The function of a point outside A–P does not have more affinity to the function of this segment according as it is nearer to it.

Comparing the three criteria of "arbitrariness", "singleness of reference" and "discreteness of elements", I believe that the latter is the more important one. This may be illustrated by means of a fairly complicated example. In the type of Dutch spoken by this author, the spirants do not participate in the opposition voiceless : voiced. The distinction is phonemic, however, in other types of Dutch, and – which is more important, for a speaker of the poorer dialect, unless trained in phonetics, is unable to tell a *v* from an *f* in the richer dialect – in the spelling, where we find pairs like *fee*

⁴⁷ Sapir, "The Psychological Reality of Phonemes", p. 47. The following quotation is from the article "Sound Patterns in Language" (1925), also reprinted in *Selected Writings* . . . , pp. 33-45.

"fairy" : *vee* "cattle", *fier* "high-spirited" : *vier* "four". A person whose name is *Feen* sometimes introduces himself: "Feen with an *f*", or he exaggerates the voicelessness (or the fortis character) of the *f*. So, in a dialect where voiced and voiceless spirants are not opposed to each other, a speaker may nevertheless use them to distinguish two words, but he has to use extreme forms and usually concomitantly lengthens the consonants to indicate that he wants these qualities to be noticed and to be interpreted as differential. When we shift the word *vee* with its initial elements along the dimension voiced – voiceless, we find in the author's dialect at one extreme end clearly "cattle", then an area where both interpretations are equally appropriate, and finally at the very end a small portion where only "fairy" is admissible. Within the middle segment, we can distinguish various pronunciations which are phonemically more or less voiced, but – and this is the most important point – which are felt as absolutely normal pronunciations of both words, whereas the small extreme ends of the dimension, though of greater distinguishing power, are definitely less normal (even in the mouth of those for whom the voice opposition does function). When a real linguistic opposition is consolidated within a dimension, we find also two (or more) unambiguous segments, but the stretch in between will be a no man's land, i.e. it leaves the decoder with a phonemic non liquet: the listener is not certain what form he has heard, he will assume that he has missed a nuance or that the speaker has not sufficiently carefully articulated. In the Dutch example, however, the uncertainty is of a purely semantic nature. Here the listener does not have the impression that the sound distinctions fail to function properly, for he does not expect any such distinctions. Thus we conclude that in the dialect in question the words *vee* and *fee* are homonyms, and we reject the phonemic opposition /v : f/. Likewise, all Americans can probably use distinctively [t] and [d] between a stressed and an unstressed vowel, but nonetheless there is a variant of American English for which it would be wrong to postulate a phonemic opposition in this environment.⁴⁸

⁴⁸ See Jakobson & Halle, *Fundamentals* . . . , p. 6.

It need not be stressed that the criterion "singleness of reference" does not help to exclude such oppositions, nor does the conception that arbitrariness of the form-meaning relationship points unequivocally to a phonemic distinction. Therefore we should give preference to the demand of discreteness.

When a speaker employs a distinction which is not relevant in his own dialect, we have of course a case of "switching the code", but there must be a means for the investigator to reveal such cases: we cannot simply ask an informant whether he uses two codes promiscuously. Employing the procedure proposed, we are able to detect distinctive uses of nondistinctive sound differences and this discrepancy exposes the intermingling of two systems.

The example of Dutch *vee* and *fee*, *vier* and *fier* has been chosen in order to show the inadequateness of current theories. The use of nonrelevant features for distinguishing words is commonly dealt with in phonemic literature. Phonemes marked by such a feature are said to occur only in "marginal words". Instead of the criterion of discreteness, current phonemics employs marginality as a criterion for consigning these phonemes to a place outside the system. In other words, all marginal elements are simply put aside when a language is studied for the purpose of setting up the phonemic system. This means that we should exactly know which elements are marginal. But it is open to question whether marginality is always a matter of direct observation. This applies also to the outer conditions under which, according to Pike, "extrasystematic phonemes" occur: they are "restricted (1) to specific styles of speech, or (2) to specific types of social situations, or (3) to specific morphemes which in turn are restricted in usage".⁴⁹

Four categories of words are generally considered marginal: onomatopoeic words, interjections, loanwords and proper names. Among the sound relationships based on marginal words we find exact parallels of Dutch [v] versus [f]. We take again a Dutch example: the English word *team* belongs to the active vocabulary of every Dutch speaker, and many people who are afraid of making

⁴⁹ Pike, *Phonemics* . . . , p. 143.

the impression of uneducatedness, have acquired the habit of distinguishing the long [i:] in [ti:m] from the ordinary Dutch short [i] in [rim] *riem* "thong, belt". The status of this difference is exactly like the one of *v* versus *f* in the preceding example. The criterion of discreteness eliminates it from the phonemic system and yields a description according to which the phoneme /i/ in *team* is, for nonlinguistic reasons, often actualized as [i:] (while elsewhere it is nearly always [i]) and the phoneme /f/ in *vier* is, for the same reasons, sometimes actualized as [v] (while usually it is [f]). It further leads to the conclusion that the language system forbids the addition of new phonemes * /i:/ and * /v/, for it does not allow them to become discrete and unambiguously distinctive. In other parts of the sound continuum, however, the same language system leaves room for enlarging the inventory of phonemes. Only those elements can assume a distinctive status which are sufficiently remote from the sound areas occupied by already existing phonemes. In this respect there are hardly any limits to the capacity of a language. Examples are Dutch nasal vowel, as in *restaurant*, or the bilabial trill in Russian *tpрукat'* (phonetically, in the notation of Isačenko,⁵⁰ [ψukat']) "to stop a horse by crying [ψu]". Therefore it seems wise to maintain the criterion of marginality, in spite of its theoretical insufficiency, but to employ it only after the criterion of discreteness. The inverse order blocks up an important source of information about the language system.

When we employ the criterion of discreteness first, we find that a phoneme border must be assumed between the Russian labial flap and the other labials (*p*, *b*, *f*, etc.) and also between this sound and the other flap (*r*). Only then we take into account the marginal character of the words (because they are derived from an interjection) in which [ψ] occurs and conclude that the sound casually fills up an empty pigeonhole in the system. We might speak, with Milewski⁵¹ (p. 45) of a "possible phoneme", in contradistinction to "existing phonemes". In the case of Dutch [i:] versus [i], however,

⁵⁰ A. V. Isačenko, *Fonetika spisovnej ruštiny* (Bratislava, 1947), p. 112.

⁵¹ T. Milewski, "Derywacja Fonologiczna", *Bulletin de la Société Polonaise de Linguistique*, IX (1949), pp. 43-57.

we do not discover a phoneme border (because there is no discreteness), and therefore the question whether or not the given form is marginal, is scarcely of interest to the phonemicist. This applies also to Dutch [f] versus [v], so that we need not consider here the possibility of marginality. And precisely in such cases the criterion of marginality would fail entirely, for neither *fier* nor *vier* can be regarded as belonging to one of the four categories of marginal elements. (To be sure, *fier* is of foreign origin, but only persons interested in etymology realize this.)

It follows that there are possibilities of distinction which are not utilized by the normal system. A description which omits them is not complete. This means that we should indicate in the analysis not only the borders (or oppositions) between the phonemes, but also the borders between the phonemes and the unsystematized sounds, i.e. the outer borders of the system as a whole. Any language is capable of incidentally utilizing the empty areas, of gap-filling.⁵² It must be stressed that this capability characterizes a language at a given stage of its development, it is a purely synchronic property.

Let us examine one more example. We take Hockett's system for Dutch: "Dutch has a four-by-three with no leftovers:

p t k
b d g
f s x
v z γ;

/g/ is quite rare (except in loanwords)."⁵³ This system is not free from simplifications: the difference between /x/ and /γ/ passes unnoticed even for a part of the speakers who keep apart /f/ and /v/. So we should speak rather of one of the varieties of Dutch than of the Dutch language in general. The borrowed phoneme /g/ is indeed extremely rare, for a majority of the loans enter the language through the spelling, so that a foreign [g] is more often replaced by /γ/. Where a spoken source prevails, however, many speakers

⁵² Martinet, "Phonetics and Linguistic Evolution"...., p. 270.

⁵³ Hockett, *Manual*...., p. 116.

simply confuse /k/ and /g/. But even for the variety which clearly distinguishes them – where, in other words, *goal* and *kool* ‘coal, cabbage’ are not homonymous –, many linguists prefer to state that the /k/ is voice-irrelevant, just because /g/ is attested only in loanwords. I am ready to admit that there is no real /g/ (principle of marginality), but I would like to maintain that, nevertheless, the /k/ of this variety carries the relevant feature voicelessness (principle of discreteness). A system set up in this way indicates the presence of an empty pigeonhole, and thus contains fuller information than we would get from the statement that the velar stops neutralize the voice opposition, or – a view entertained by other linguists – that voiced velars neutralize the stop-spirant opposition. It should be noted that current theories are incapable of declaring the one solution more appropriate than the other. Indeed, if there are three phonemes /k, x, γ/, it is hard to decide whether /γ/ has an irrelevant degree of closure, or /k/ is not characterized as to voice. The procedure of this study requires that we examine how a [g] would be interpreted by speakers of such a dialect. The speakers of one type of Dutch unquestionably perceive [g] as a somewhat strange realization of /k/, and never as /γ/. This argument should in my opinion tip the balance, and /k/ should in such dialects be described as voice-irrelevant.

A similar example from Russian is adduced by Šaumjan,⁵⁴ an adherent of Jakobson’s theories. He states that the Russian phoneme /x/ is phonetically voiceless, but this voicelessness cannot serve as a differentiating feature, because there is no voiced counterpart in Russian. Here the author disregards the important fact that a pronunciation of /xot/ *xod* ‘act of going, move’ with voiced initial consonant, i.e. [γot], is interpreted by a Russian as a different word, to wit /got/ *god* ‘year’. Therefore I consider Šaumjan’s approach too mechanistic.

Already in 1949 Milewski⁵⁵ directed the attention to the fact

⁵⁴ S. K. Šaumjan, *Istorija sistemy differencial’nyx elementov v pol’skom jazyke* (Moscow, 1958), p. 16.

⁵⁵ Milewski, ‘Derywacja...’.

that it cannot be said of any living language with full certainty and exactness how many phonemes function in it (p. 43). The phonemes cannot be counted (p. 44). The reason is, according to Milewski, that a phonemic system belongs to the "open systems" (p. 45), which means that the addition of certain new entities does not change the system (p. 50). As said above, Milewski distinguishes in this way "existing" or "normal" phonemes and "possible" or "potential" phonemes (pp. 45, 52f). This addition is called "derivation", because the potential phonemes fill open spaces in proportions (correlations), such as the Polish opposition nonpalatalized versus palatalized " $p : p' = b : b' = t : (t') = d : (d')$ " (p. 51; Milewski places the "possible" phonemes in parentheses). This possibility is restricted to the correlations of privative oppositions (in Troubetzkoy's terminology), where one or more positive terms are lacking in the normal system (pp. 47ff).

The last sentence of this summary of Milewski's views shows a criterion different from the one proposed in this study. The palatalized $[t']$ is, according to Milewski, possible in Polish because it belongs to a well-defined type of correlation. The criterion of discreteness, on the other hand, enables us to conclude that the place of $[t']$ is open in the Polish system on the basis of the observation that the existing $/t/$ is clearly felt as unpalatalized by the natives. On closer inspection, Milewski's criterion again turns out to be a roundabout way of learning something about the interpretation of the natives, whereas I would prefer to consult the speakers themselves. It cannot be denied that Milewski's method rests on more objective facts and is therefore better founded. However, it is sometimes misleading, proof of which is furnished by the example of Dutch $[g]$. Although it belongs to the required type of correlation — $p : b = t : d = k : (g)$ — its place in the sound continuum is in one variety of Dutch a part of the area occupied by $/k/$ and consequently not empty. The interpretation of the speakers of this dialect does not leave room for doubt in this respect.

Milewski was not the first to signalize these facts. He himself

mentions the studies of Malmberg.⁵⁶ Malmberg's point is that a too consistently synchronistic approach leads into a deadlock. We should take into account not only the system, but also its tendencies. He distinguishes oppositions and "nuances". A careful reading shows that he uses the latter term in two different senses. In the first place it is a doubtful sound difference corresponding to a difference of meaning, such as French *e* : *ɛ*, usually neutralized before the accent, but potentially distinctive in the pair *nous pêchons* : *nous pêcheons*. With the word "doubtful" I summarize Malmberg's statement: "Les quelques cas [...] ne sont pas assez nombreux et ne nous semblent pas suffisamment sûrs pour permettre de baser là-dessus un schéma phonologique" (II, p. 238). Perhaps it can be inferred that the difference is of the continuous-scale type, and then the discreteness criterion will reveal its non-phonemic character. The second meaning of "nuance" in Malmberg's articles is: a sound difference not corresponding to a semantic difference, although capable of doing so in similar contexts, such as French *e* : *ɛ* in final position in a monosyllabic word, sometimes distinctive (*clef* : *claie*), "mais une simple nuance ou variation dans [se] : [sɛ] (sc. *sait*)" (II, p. 244). It seems that Malmberg relies too much upon meaning as a criterion for establishing distinctiveness. Nothing prevents us from regarding the two pronunciations of *sait* as phonemically different, exactly like English [ægein] and [ægen], or from considering *pêchons* and *pêcheons* homonymous forms, despite their semantic difference. "Nuances" are then nonphonemic sound differences capable of casually distinguishing meanings. Acceptance of such nonsystematical features helps us surmounting the difficulties which current linguistics finds on its way when attempting to set up a purely synchronistic phoneme system. If an experiment makes it clear that the feature opposing *claie* to *clef* is of this type, the French vowel system will be identical with "le schéma qui représenterait

⁵⁶ B. Malmberg, "Observations sur le système vocalique du français", *Acta Linguistica*, II (1940-41), pp. 232-246; "A propos du système phonologique de l'italien", *Acta Linguistica*, III (1942-43), pp. 34-43; "Bemerkungen zum quantitativen Vokalsystem im modernen Französisch", *ib.*, pp. 44-56.

les tendances du vocalisme français et qui serait sans doute réalisé, si le jeu de ces tendances était libre" (Malmberg, II, p. 243), i.e. a system with only one /e/.

Nondiscrete features often present themselves as potential features, such as length in French ϵ versus $\epsilon:$. Malmberg writes: "Der Unterschied zwischen *tête* und *tette* besteht nicht darin, dass das erste Wort einen langen und das zweite einen kurzen Vokal hat, sondern darin, dass der Sprechende eine Möglichkeit besitzt, das normalerweise kurze ϵ in *tête*, wenn es der Zusammenhang aus dem einen oder andern Anlass fordert, zu verlängern, und dies ist nicht der Fall bei *tette*" (III, p. 50). Although it cannot be claimed that potentiality in this sense points unequivocally to nondiscreteness, it certainly is a strong indication of it.

There is still another reason for acknowledging the superiority of the criterion based upon discreteness. Some intonation patterns characterizing a sentence have "singleness of reference" and are nonetheless distinctive. As a rule a sentence intonation has two functions. In the first place it marks a group of words (or one word) as a linguistic unit, a sentence. In this, demarcative, function the intonation pattern is no doubt distinctive, for the same words occur also without it as a part of a sentence: it distinguishes sentences from nonsentences.

Secondly, the same intonation patterns characterize types of sentences. Here the discreteness criterion helps us to draw paradigmatic borderlines. Two sentences with the same word content (in the same order) are separate linguistic units when their intonation difference is discrete. In the opposite case they are just variants of the same linguistic form. Jakobson, following Harris, singles out three intonations which are neither "expressive" nor redundant: "/." denotes the end of the sentence, "/," the end of a phrase in a sentence to be continued, and "/?" the question, which in configurational terms means the end of a sentence to be supplemented by an answer".⁵⁷ Other patterns give information about the mood of the speaker, such as amazement, anger; they are "expressive"

⁵⁷ Jakobson, Fant & Halle, *Preliminaries* . . . , pp. 14f.

features. Jakobson's remark that "expressive" features are characterized by a "grading gamut" (*ib.*, p. 15) indicates that he acknowledges in this case the discreteness criterion.

It should be noted, however, that his conclusions are sometimes different from the views put forward in modern literature on the subject. Hockett⁵⁸ presents for a language like English nearly a hundred distinctive intonations, in our eyes a rather large number, but at any rate definitely meant to be finite. In this respect I find it difficult to follow him. It is just a question of assiduity to ascertain still more intonations, all of them "culturally organized". Why not, for instance, assume a number of seven pitch levels instead of three or four? Any degree of rise or lowering causes a certain effect, which is easily interpreted as correlated to a special mood of the speaker. It is possible that Hockett has gone through a considerable amount of experimentation before reaching his conclusions, but his results corroborate the view that a discreteness test is indispensable. At least one of Hockett's distinctions does not meet the requirements: ²*It's three o'clock*↓ versus ²*It's three o'clock*↓ (compared by Hockett with the French "accent d'insistance") is a clear example of a "grading gamut".

Martinet, while speaking of sentence intonation, says that it "stands outside of the field of linguistic arbitrariness".⁵⁹ He also mentions discreteness, but it seems that he does not properly distinguish the two phenomena, that he regards the one as a symptom of the other. He describes discreteness in the given instance as something introduced by the linguist: "An attempt has been made to reduce [intonation] to a certain number of discontinuous elements, which reduction, in all likelihood, does not agree with its real nature and function". Here lack of arbitrariness appears as a criterion. However, if we agree on taking discreteness as an observable phenomenon (provided we dispose of a reliable manner of detecting it), it can serve as a criterion itself. Then it by no means coincides with arbitrariness.

⁵⁸ Hockett, *Manual*..., § 211, pp. 45-51.

⁵⁹ Martinet, "Phonetics and Linguistic Evolution"..., p. 261.

As to "singleness of reference", it is further adduced by Jakobson and Halle as a proof of the nondistinctiveness of redundant features. I quote: "Redundant features help to identify a concurrent or adjoining feature",⁶⁰ and this is their "single specific denotation". The examples are taken from Russian (p. 10): "The first vowel of /pil,il/ is a velar /u/ in contradistinction to the palatal [i] of /p,il,il/ and this difference between back and front is a redundant feature pointing to the distinctive opposition of the preceding unpalatalized (plain) and palatalized (sharp) consonant." Jakobson has more than once called the attention to the fact that under special circumstances redundant features assume the function of distinctive features. Thus in the example the timbre [i] of the first vowel can inform a listener about a quality of the preceding consonant when this quality itself escapes his notice or is made unrecognizable by the transmission channel. There is no danger, however, that the procedure of the present chapter will lead to misinterpreting the redundant feature as distinctive, for the experiments reveal as distinctive only cavity friction, high tongue position and unrounded lips. The question whether these features belong to the first or the second phoneme of a word is answered by the second step of the procedure (Chapter II of the present study), which serves to establish how the features are combined into phonemes. We need the criterion of "singleness of reference" only if we assume that the palatalization of the consonants and the timbre of the vowel represent two separate consecutive features in the chain of sound instead of one, but this is in my opinion an a priori assumption; at least Jakobson and Halle do not adduce any arguments for it.

Among the redundant features, the border marks take a place apart. The first type of this phenomenon does not work by means of redundant features, it is only a question of phoneme distribution: a cluster of phonemes which occurs only partly on one side and partly on the other of a word or morpheme boundary, may be

⁶⁰ Jakobson & Halle, *Fundamentals* . . . , p. 9.

⁶¹ See for a detailed discussion of the subject Troubetzkoy, *Grundzüge* . . . , pp. 241-261 (= *Principes* . . . , pp. 290-314).

said to signal this boundary.⁶¹ This type is not important for phonemic analysis, nor does it help the investigator to isolate words or morphemes in the chain, for he must first have discovered these units by means of other criteria before he can recognize a phoneme cluster as a carrier of a border signal. Furthermore, we must not forget that the significance for the listener is impaired by the fact that marginal words usually do not conform to the rules of distribution, so that in a conversation the presence of a border is never made entirely certain in this way. The same applies of course to single phonemes and even distinctive features which, because of their limited distribution, signalize presence or absence of a boundary.

Border marks of the other type are redundant features. They pose a problem to the phonemicist. Jakobson and Halle give a very clear example: "If we compare two sequences /krugóm pil,fl/ '(one) spread dust all around' and /ispómpil,fl/ '(one) poured from a pump,' we observe that the syllable /pi/ in the second specimen contains a more obscure (tending toward a brief mid-central articulation) variety of vowel than in the first sample. The less obscure variety appears only immediately before the stressed syllable of the same word and thus displays a configurative feature: it signals that no word boundary follows immediately."⁶² Furthermore, we often observe a difference in the realization of a phoneme cluster according as it is split or not by a morpheme or word border. For instance, in Polish the cluster /ts/ has usually not the same pronunciation when the two phonemes belong to different morphemes (as in *odsadzac*) and when they are not separated by a morpheme border (as *c* in *ocalić*). This is the reason why Trager⁶³ assumes for the Polish system a phoneme /c/ distinct from /ts/. It is clear that the elements [c] and [ts] contain a relevant difference, but the morpheme border and the phonetic difference cannot both be distinctive, since the one is a consequence of the other and the relationship between them is not semiotic

⁶² Jakobson & Halle, *Fundamentals*..., p. 10.

⁶³ G. L. Trager, "La systématique des phonèmes du polonais", *Acta Linguistica*, I (1939), pp. 179-188 (p. 179).

(significans/significatum). A descriptive linguist should avoid giving repeated information. If a sentence is ultimately described as a string of words, morphemes and phonemes in a given arrangement, we can choose one out of two possible phonemic notations of Polish [c] and [ts], either /ts/ and /t-s/ (with /-/ standing for a morpheme border) or /c/ and /ts/ (with the implication that the cluster /ts/ signals a border). A notation /c/ and /t-s/, however, would give the same information twice. Since a morpheme border is relevant in a great many cases where no border signals can replace them in the description, only the first notation is defensible. The realizations [c] or [ts] are contextually conditioned, they are variants of the same phonemic form.⁶⁴

Redundant features functioning as border marks are often responsible for so-called "internal open juncture".⁶⁵ It may be remarked, however, that in all examples enumerated by Hockett except one (p. 63), the author inserts in his statements one of the adverbs "most usually", "normally", "frequently (though not always)", "seldom if ever", "often". Similar restrictions are extremely rare when a distinction of, for instance, front and back articulation is discussed. I conclude that juncture is not obligatory. As we learn from experience, potential features are often at the same time nondiscrete (see above, p. 53). If we apply the discreteness criterion, the number of distinctive junctures will be smaller than Hockett assumes. It goes without saying that we can speak, with Pike, of a "grammatical phonological border point" only when the requirement of paradigmatic discreteness is fulfilled on the phonemic level as well as the requirement of syntagmatic discreteness on the level of the morphemes. The /-/ in Polish /t-s/ is an example.

To summarize: the demand of discreteness furnishes the most important criterion for establishing the phonemic inventory of a language. It is valid for any property of sound. It enables us to

⁶⁴ See K. L. Pike, "Grammatical Prerequisites to Phonemic Analysis", *Word*, III (1947), pp. 155-172; "More on Grammatical Prerequisites", *Word*, VIII (1952), pp. 106-121; *Phonemics*..., pp. 159ff.

⁶⁵ Hockett, *Manual*..., p. 52.

decide unequivocally whether a given feature belongs to the code. After it, as a second step, the criterion of marginality has to be employed. The other criteria may add to the picture obtained in this way only the nondiscrete potential features, which are of secondary importance.

SYNTAGMATIC DELIMITATION OF PHONEMES

1. Troubetzkoy writes in his *Grundzüge der Phonologie*: "Phonologische Einheiten, die sich vom Standpunkt der betreffenden Sprache nicht in noch kürzere aufeinanderfolgende phonologische Einheiten zerlegen lassen, nennen wir *Phoneme*. Somit ist das Phonem die kleinste phonologische Einheit der gegebenen Sprache."¹ In this definition the word "kleinste" has caused serious difficulties to nearly everybody who has occupied himself with the study of phonemics. For Milewski this is one of the two reasons why he thinks the number of phonemes in a given language cannot be counted.² The problem is indeed so hard to solve that, in his paper on the subject,³ Hintze is forced to withdraw before it and to sacrifice for the sake of a solution the basic principle of phonemic analysis, which is to "set apart and give prominence to those phonetic differences which are sufficient to distinguish one word or one grammatical form from another".⁴ Hintze writes: "Es muss nun zugegeben werden, dass [...] die Gefahr eines *circulus vitiosus* nahe zu liegen scheint: Die Silbenstruktur definieren wir auf Grund unserer Auffassung der phonematischen Einheiten, – diese selbst sollen nun aber bei Troubetzkoy gerade wieder durch die Berücksichtigung der Silbenstruktur bestimmt werden. Mir will scheinen, dass man diesen Zirkelschluss nur durch eine angemessene Berücksichtigung gewisser *phonetischer* Gegebenheiten entgegen kann." In other words: purely phonetic criteria are, according to Hintze, indispensable for a solution of the problem of monophonematic interpretation.

¹ Troubetzkoy, *Grundzüge* . . . , p. 34 (= *Principes* . . . , pp. 37f).

² For the other reason see above, pp. 50-51

³ Hintze, "Zur Frage . . . ",

⁴ Martinet, *Phonology* . . . , p. 1.

Before Hintze's paper the situation was hardly better. We do not have a single study in which the author was able to avoid the use of phonetic considerations in this part of phonemic analysis. Martinet mentions, as a shortcoming of Troubetzkoy's *Grundzüge*, the way the problem "one or two phonemes?" is solved. He writes: "Nous trouvons [...] le problème résolu, à notre sens, de façon peu satisfaisante, à l'aide de critères dont la valeur, en matière de phonologie, n'a pas été éprouvée précédemment".⁵ Troubetzkoy gives, among others, the following rules: I. A phoneme may not belong partly to one syllable and partly to another. II. A phoneme may not comprise more than one articulatory movement. III. A phoneme may not exceed a certain length.⁶ Martinet calls these rules "negative", "des restrictions purement phonétiques", and he does not accept them.⁷ Meanwhile his own theory is not entirely free from phonetic considerations, either, as I hope to show presently.⁸

As a matter of fact, Troubetzkoy was more anxious to find a nonphonetic foundation for his analysis than later writers seem to have noticed. In this regard the following passage is typical: "Phonetisch betrachtet, besteht jedes *b* [in German] aus einer ganzen Reihe artikulatorischer Bewegungen: zuerst werden die Lippen einander genähert, dann aufeinandergelegt, sodass der Mundraum von vorne ganz geschlossen ist; gleichzeitig wird das Gaumensegel gehoben und gegen die Rachenwand gestemmt, sodass der Eingang aus dem Rachen in den Nasenraum versperrt wird; gleich darauf beginnen die Stimmbänder zu schwingen, die aus der Lungen heraufströmende Luft dringt in den Mundraum ein und staut sich hinter den Lippenverschluss; endlich wird dieser durch den Luftdruck gesprengt. Jede von diesen aufeinanderfolgenden Bewegungen entspricht einem bestimmten akustischen Effekt. Keines von diesen 'akustischen Atomen' kann aber als

⁵ A. Martinet, "Un ou deux phonèmes" . . . , p. 95.

⁶ Troubetzkoy, *Grundzüge* . . . , pp. 50ff. (= *Principes* . . . , pp. 57ff).

⁷ Martinet, "Un ou deux phonèmes" . . . , p. 100.

⁸ In spite of Cantineau's remark in his French translation of *Grundzüge* . . . : Troubetzkoy, *Principes* . . . , p. 57n.

phonologische Einheit betrachtet werden, weil sie immer alle zusammen, niemals isoliert auftreten: auf die labiale 'Implosion' folgt immer die 'Explosion', die ihrerseits immer von der Implosion eingeleitet ist, und der labial gefärbte 'Blählaut', der zwischen der Implosion und der Explosion ertönt, kann ohne die labiale Implosion und die Explosion nicht vorkommen. Somit ist das ganze *b* eine phonologische, zeitlich nicht zerlegbare Einheit."⁹

Indeed, the explosion is phonemically of no importance: it usually accompanies an implosion, and if an implosion occurs without it, we have a variety which does not affect the meaning of the word. But a "Blählaut" may be preceded by various implosions which also occur without it, and presence versus absence of "Blählaut" is a relevant difference. A phonemicist will not regard the labial color of the "Blählaut" as an argument against listing it as a separate phoneme, for, to take an other case, nobody would think of presenting the group [ti] as one phoneme just because an *i*-colored [t] occurs only in combination with [i]. So why should we not consider the "Blählaut" a phoneme? One might adduce against this the argument that the distribution of the "Blählaut" is extremely limited. But if such a limited distribution would forbid to consider a sound a phoneme, we could not see separate phonemes in sounds like *h* and *ŋ*, either, and, consequently, the word *hang* would, as a whole, constitute a single phoneme. Yet nobody has probably ever taken this stand. And if Troubetzkoy mentions the fact that the "acoustic atoms" never occur isolated, the same can be said of nearly all German phonemes.

No matter what we think of Troubetzkoy's solution, his procedure is different from the methods of his successors in that he thinks it necessary to establish the *phonemic* unity also for phonemes like /b/. Later studies deal only with the doubtful interpretation in cases such as /ts/ or /c/, /th/ or /t^h/, /tj/ or /t'/, /tw/ or /t^w/, /aN/ or /ã/. Martinet, who has dedicated a separate paper to the problem, starts from an *objective* (*phonetic*) difference between articulations, units of sound, and groups of sounds. When by means of phonetic

⁹ Troubetzkoy, *Grundzüge* . . . , p. 33 (= *Principes*, pp. 37f).

criteria has been decided which sounds might be subject to a biphonematic interpretation, Martinet requires that the further, phonemic, analysis be exclusively based on considerations of distinctive function; he follows, what he calls, the method of "commutation". He gives a set of rules, which may be resumed like this: only when both of the sounds of a group in all positions in the word where the group occurs can be replaced by another sound, so that an other word is obtained, Martinet regards the group as consisting of two phonemes.¹⁰

In this manner the first step in phonemic analysis is a phonetic classification of the sounds as certainly monophonematic or potentially biphonematic elements. Pike is very plain about it: he gives a list of "Segment Sequence Types which are Suspicious because They Might Prove to be One or Two Phonemes".¹¹

Besides the functional criterion we sometimes find also employed a structural criterion: suppose in a given language there occur in a given position in the word only single consonants or certain potentially biphonematic complexes (*ts*, *th*). In this case many linguists think the phonetically simple sounds more important for structural conclusions than the potentially biphonematic sounds: they take into account only the phonetically simple sounds for establishing the types of word structure the language under study exclusively allows. According to this principle they derive a structural rule (e.g. "in this language there occur initially only single consonants") before they have studied the behavior of the sounds *ts*, *th*, etc. The phonetically complex sounds have to fit into this structure at any price and are phonemically reshaped accordingly. i.e. *ts* > *c*, *th* > *tʰ*, etc.: they are interpreted as single phonemes.¹² But what is against a formulation like: "In this language there occur initially only single consonant phonemes and a limited number of groups of two consonants, let us say, those

¹⁰ Martinet, "Un ou deux phonèmes" . . . , pp. 95ff.

¹¹ Pike, *Phonemics* . . . , p. 131; see also pp. 139f.

¹² "Rule IV" of Troubetzkoy, *Principes* . . . , pp. 60f.; Martinet, "Un ou deux phonèmes" . . . , pp. 99f.; A. Martinet, "Occlusives and Affricates with Reference to Some Problems of Romance Philology", *Word*, V (1949), pp. 116-122 (p. 117); Hintze, "Zur Frage . . . ", *passim*; Pike, *Phonemics* . . . , pp. 131f.

consisting of a stop plus the homo-organic fricative (or even one such a group)”?)

If it were completely clear to everybody in what cases we have to regard a complex sound as potentially biphonematic, the question would not be worth while speaking about. But it sometimes happens that two trained phoneticians set forth a different opinion in one and the same case. In one of his studies Roman Jakobson describes the Serbocroatian phonemes.¹³ We may assume that he regards phonemes as bundles of simultaneous distinctive features, but it is not easy to find out in what sense we have to understand „simultaneous”. Jakobson distinguishes an “axis of successiveness” (along which the phonemes are arranged within the word) and an “axis of simultaneity” (along which the distinctive features are grouped within the phoneme). One of the “features concerning the axis of simultaneity only” is the feature of “continuousness”. Jakobson “mark[s] by a plus sign only the presence of the feature in question; the absence (as its opposite) is indicated by a minus sign only there, where no plus sign occurs at all. A complex combining both opposite terms is represented by the \pm sign” (pp. 108ff). Here follows a part of his diagram:

	ć	č	š
Continuousness		\pm	+

In other words, the phoneme /č/ is marked as a complex combining both the absence and the presence of continuousness, absence as compared with /š/, presence as compared with /ć/. In the same way /e/ is marked as a complex combining both presence and absence of “saturation”, against /i/ (which has absence of the same) and /a/ (which has presence). It is clear from the latter example that the term “complex” cannot be connected with a possible biphonematic interpretation.

If we read what Serbocroatian scholars write about their affricates, we find: “č = kakuminalno t + š”.¹⁴ The “cacuminal t”

¹³ Jakobson, “On the Identification . . .”.

¹⁴ T. Maretić, *Gramatika i stilistika hrvatskoga ili srpskoga književnog jezika* (Zagreb, 1899), p. 27.

is slightly different from the *t* in other positions, the *š* is identical with the *š* in other positions.¹⁵

We see: what one scholar calls "presence of continuousness" (a *feature* by which the phoneme as a whole is characterized), is for another the second *part* of a complex sound. If this discrepancy were a result of the difference between a phonemic and a phonetic approach, we would not object to it. But the idea of the unity of Serbocroatian *č*, which we find in the study of Jakobson, is not based upon phonemic principles, but upon phonetic observation. Those who adhere to these views of his are so convinced of its unity that they even refuse to subject it to the criteria laid down by Martinet in the paper "Un ou deux phonèmes". In fact, if they did, it would turn out that they would have to break it up into two phonemes, because the Serbocroatian *č* does not seem to meet any of Martinet's requirements for monophonematic interpretation. The "cacuminal *t*" would be a special realization of the phoneme /*t*/, conditioned by the following /*š*/.

There are persons for whom the phonemic interpretation of Serbocroatian *č* as /*tš*/ is exactly as unacceptable as would be for others the interpretation of German *b* as /*p8*/, in which /*8*/ is chosen to indicate the "phoneme" voice.¹⁶ But if one thinks it necessary to doubt the phonetic feelings of somebody else, one should doubt his own phonetic intuition as well. Consequently, we ought to check the monophonematic character of German *b* by means of the same criteria put forward by Martinet. So we subject German [*p8*] to the experiment of commutation. We take the word *Beil* [*p8ajl*]; the [*p*] is commutable: we have the word *Seil* [*s8ajl*]; for [*8*] we may substitute [*f*]: *Pfeil* [*pfajl*]. In another word we may substitute for [*8*] also zero: *Bein* [*p8ajn*] – *Pein* [*p0ajn*]. Nevertheless there is no doubt that, according to Martinet's views at the time he wrote his "Un ou deux phonèmes", there is an indivisible German phoneme /*b*/.

¹⁵ A. Belić, *Savremeni srpskohrvatski književni jezik*, I (Beograd, 1951), pp. 89, 94, 101.

¹⁶ This interpretation would not be, even phonetically, as strange as it seems; compare Troubetzkoy's discussion of the "Blählaut" in the beginning of this chapter.

Thus, for Martinet, there are three cases: 1. When a sound, from a phonetic viewpoint, is potentially biphonemic, but, according to the criterion of commutation, a unit, it is interpreted monophonematically (e.g. German *z*, [ts] = [c]). 2. When a sound, phonetically, is a unit, but, according to the criterion of commutation, has to be interpreted biphonematically, it is also regarded as one phoneme (e.g. German *b*). 3. Only when a sound is neither a phonetic unit, nor a unit according to the criterion of commutation, it is interpreted biphonematically (e.g. probably Serbocroatian *č* = [tʃ]).

The result is a mixture of phonetic and functional criteria, and, for that reason, difficult to accept.

Another criterion which might be employed in this question is Hjelmslev's requirement of greatest possible simplicity of description.¹⁷ It has been discussed by Eli Fischer-Jørgensen.¹⁸ Fewer phonemes in the inventory means more phonemes in the chain, and, reversely, a lower number of phonemes in the chain implies a higher number in the inventory. So it is not decisive. We may choose the former possibility and identify the phonemes with their relevant features, or we may choose the latter and consider the [u] from a group [pu] just a distinctive feature of the phoneme /p^u/.¹⁹

2. It is clear that our problem is far from solved. It seems that all thus far developed techniques of phonemic analysis have something in common which makes a solution of the question of monophonemic interpretation impossible. I am convinced that we must look for it in the order of the successive steps of the analysis, which first dissects the chain into phonemes (micro-phonemes) and only after that ascertains the distinctive features. When the analysis advances in this direction, it will be impossible to decide when the former step ends and the latter begins.

Even the number of distinctive oppositions in the system may be

¹⁷ Hjelmslev, *Omkring* . . . , p. 12 (= *Prolegomena* . . . , p. 6).

¹⁸ E. Fischer-Jørgensen, "Remarques sur les principes de l'analyse phonémique", *TCLCopenhagen*, V (1949), pp. 225ff.

¹⁹ Compare Martinet, *Phonology* . . . , p. 16; Hockett, *Manual* . . . , p. 160.

influenced by the way we solve the problem of monophonematic interpretation. Martinet describes the procedure: "If we [...] compare *make it* with *take it*, we notice that the differentiating elements are [...] *m-* and *t-*. But we cannot carry our analysis any farther; whatever English expression we choose to compare with *make it*, we cannot divide *m-* into successive elements, and this indivisible whole we call a phoneme or more exactly the realization of a phoneme. It is true that if we adduce the expression *bake it*, we notice that its first element has several features in common with that of *make it*, for instance a similar action of the glottis and a same lip-closure. As a matter of fact, the keeping apart of *make it* and *bake it* is secured by a different play of the velum. This we call a 'relevant feature'." ²⁰

Now it all depends on when we think we cannot go farther with the phonemic analysis. Let us take the example of Polish *trzy* and *czy*. Both might be described [tʃi], yet they are phonetically and phonemically distinct. If we continue the analysis until we reach [tʃi], we can apply Martinet's criterion of monophonematic analysis and describe the one as two phonemes /tʃ/, the other as a single phoneme /č/. ²¹ But if we think we ought to stop at [či] (as Jakobson does with regard to the Serbocroatian [č]), we are compelled to introduce one more relevant feature in the Polish system, to wit an other place of articulation. Otherwise we cannot describe the phonemic distinction between /č₁/ and /č₂/.

Twaddell shows that he has noticed the difficulty: expounding his technique for detecting the relevant features, he adds as a condition: "If we have properly reckoned the phonetic fractions."²² However, he does not offer a method (other than phonetic observation) for delimiting these fractions.

The only solution we can imagine is that we change the order of the successive stages of the analysis. If we can find a method which would enable us to derive the distinctive features without previously dividing the sequence into phonemes, we could define the phoneme

²⁰ Martinet, *Phonology* . . . , p. 3.

²¹ Trager, "La systématique . . .", p. 179.

²² Twaddell, *On Defining* . . . , p. 42.

as a special type of group of distinctive features. In the first chapter I have proposed precisely such a method, which, besides the advantages mentioned there, presents a solution for the problem of monophonematic interpretation.

3. An analysis of phonemes into distinctive features is said to be necessary because otherwise we cannot identify phonemes in different environment. When we invert the order of the analysis, however, an other question arises: why are we convinced that phonemes are an indispensable part of the description of the form plane of a language? Why could we not as well describe a form immediately as a number of distinctive features in a certain order? Is there really any need of an intermediate entity, the phoneme? Or have phonemes only the practical importance that they make it easier to write a language, since it would obviously be too cumbersome to have letters for the distinctive features or to use for every morpheme a different character?

Theoretically we have a right to introduce the notion "phoneme" only when we discover bundles of distinctive features for which the fact that they are grouped together is relevant. Does it happen that the same features in the same order constitute different linguistic forms according as they are grouped together differently?

This question is not easy to answer because the notion "feature" is often not clear in the works of those who handle it. Twaddell has shown that Bloomfield's theories suffer considerably from a use of the term "feature" in "at least two different senses".²³ In later studies we still find similar situations. Jakobson, in his above mentioned paper on Serbocroatian phonemes,²⁴ speaks of vocality, nasality, voicing, etc., as "features". A phoneme is marked by the absence or the presence of these features. One phoneme, /t/, is marked by the absence of all features. When we indeed, as Jakobson requires, try to break up this /t/ into its distinctive features, the result is either zero, or /t/ is a bundle of distinctive features, each of which consists of the absence of a feature. In other words,

²³ Twaddell, *On Defining . . .*, pp. 20ff.

²⁴ Jakobson, "On the Identification . . .", pp. 208ff.

the absence of a feature is itself again a feature. This double sense of the term "feature" does not impair the value of this highly stimulating contribution to phonemic science, but it makes the paper very difficult reading. We believe that we can better stick to the terminology of an earlier article, written by the same author in collaboration with J. Lotz,²⁵ and distinguish between "oppositions" vowel: consonant, nasal : oral, etc., on one hand, and "features" as the terms of these oppositions on the other. This is what Jakobson does in his later studies.

Distinctive features, then, are not additive entities, they do not themselves constitute together a phoneme, they are not themselves fractions of speech sound, and the phoneme is not merely a fraction of speech sound of a higher order. If they were additive entities, one would indeed expect that the analysis of phonemes into distinctive features would be of the same nature as the analysis of morphemes into phonemes.

Distinctive features are qualities: they characterize a fraction of speech sound as a phoneme. Whenever an object has two mutually exclusive qualities (such as colors), we must regard it as consisting of two parts. Then, not only the presence of a quality is important, but also the extension of the quality within the object. Within the object we ought to draw a line between a part having a quality and a part marked by its opposite. In a linguistic form the parts between these border lines are the phonemes.

Let me give an example. In the English word *text* the last part [kst] contains the features: interceptedness (1) – continuousness (2) – interceptedness (1), and velarity (x) – dentality (y). We may order these features lxy21 (results: [kst] or [ktst]), or xl2yl (results: [kxt] or [kst]), etc. Briefly, there is not a single order in which the features can be arranged so as to point unambiguously at the linguistic form we try to describe.

Consequently, if we want a description to be univocal, the study of the phonemes is indispensable. For delimiting the phonemes in the chain we may start from the following considerations.

²⁵ Jakobson & Lotz, "Notes on the French....".

Within a phoneme the features play a modifying role, but the phonemes are the constitutive elements of a morpheme. Therefore the features within one phoneme may not contradict each other or be identical: they are just different. On the other hand, the phonemes in the inventory always stand in an opposition to each other: of two phonemes the one always contains at least one feature incompatible with one feature of the other. In the chain, where every phoneme occurs many times, two phonemes may be opposite to each other or identical, but never just different.

In this respect I agree with the following statement by Jakobson, with the exception of the first sentence: "The difference between the distinctive features of contiguous bundles permits the division of a sequence into phonemes. This difference may be either complete, as between the last two phonemes /i/ and /ŋ/ in the word *wing* (which have no distinctive features in common) or partial, as between the last two phonemes of the word *apt* - /p/ and /t/ all of whose distinctive features are the same except one: /p/ is grave and /t/ is acute."²⁶ An analysis of one of Jakobson's examples suffices to show that the first sentence presents the facts in a far too simple manner. Indeed /i/ and /ŋ/ in *wing* contain - for the terminology, see above pp. 33f - absence versus presence of cavity friction and absence versus presence of nasality, but this is not yet a sufficient reason for isolating them as separate phonemes, for the positive ones among these features can be combined into one simultaneous whole /ŋ'/, a palatalized dorsal nasal, realized as [iŋ]. All theories of phonemic analysis lead, of course, to rejecting this monophonemic interpretation, but thus far not on solid grounds. The current arguments in this matter are, as said above, of a purely phonetic nature. Jakobson adds the functional criterion of "difference between the distinctive features", but this apparently does not work either. Nor does it help us to prove that an analysis of *wing* as /wiGN/ would be wrong, where /G/ stands for dorsal occlusion and /N/ for nasal. /G/ and /N/ do not have a feature in common, exactly like /i/ and /ŋ/ in Jakobson's analysis (in both

²⁶ Jakobson, Fant & Halle, *Preliminaries* . . . , p. 4.

cases we disregard the voice, since it is nonphonemic). In order to eliminate the possibility of such solutions, I think it necessary to make a new attempt at solving the problem of the paradigmatic delimitation of phonemes.

In the phonemic analysis of *wing* as $/w\eta/$ the negative features are overlooked, to wit the nonnasality of $[i]$ and the lack of cavity friction in $[\eta]$. If the omission of these features is functionally not correct, we have inevitably in the segment *-ing* two pairs of mutually exclusive features, which cannot be assigned to the same phoneme. The following procedure rests on this kind of reasoning.

Suppose, we have detected in a given form the distinctive features $+A$ and $+B$: if we change $+A$ into $-A$, or $+B$ into $-B$, we obtain a different form. Now the question is: do $+A$ and $+B$ belong to one phoneme $/+A, +B \dots/$ or to two different phonemes $/+A, \dots/$ and $/+B, \dots/$?

If the latter solution is right, it is to be expected that the realization of the phoneme $/+A, \dots/$ is slightly colored by the feature $+B$ of the contiguous phoneme, but this $+B$ -color is phonemically of no importance. But if the feature $+B$, as far as it appears simultaneously (i.e. within the same phoneme) with the feature $+A$, is irrelevant, it may be omitted and even be replaced by $-B$ without causing a change in the identity of the form. Consequently, we must find out whether the word started from may be realized, as to the features in question, in either of the two following ways: 1. $/+A, -B, \dots/$ $/-A, +B, \dots/ \dots$; 2. $/-A, +B, \dots/$ $/+A, -B, \dots/ \dots$. If both interpretations are excluded (because, if we tried to pronounce these features definitely in this order, the words we would produce would sound to native speakers as forms different from the one we started from), then we have to admit that the word contains a phoneme $/+A, +B, \dots/$.

Let me give an example. Suppose, in a language we find a word $[ba]$ showing among others the features voice and interceptedness, for $[pa]$ and $[va]$ would be interpreted as different forms. Do these two features in this word belong to the same phoneme? If $+A$ is voice, then $-A$ is voicelessness; if $+B$ is interceptedness, $-B$ is continuousness. A phoneme $/+A, -B, \dots/$ may be something

like [v], [m], [z], etc., /-A,+B,.../ may be [p], [t], etc. There are in the word still other features, e.g. labiality (cf. [da]): it goes without saying that in the artificial construction either both /+A,-B,.../ and /-A,+B,.../ or at least one of them should contain also these features. If none of the artificial forms we obtain ([pva], [vpa], [mpa], [pma]) is interpreted as [ba], the conclusion will be that the features voice and interceptedness in the word [ba] belong to one phoneme; but in the opposite case, when [ba] and, say, [vpa] are interpreted as realizations of one form, we choose /vp.../ as the most adequate phonemic analysis.

In this analysis the phoneme /p/ does not have the feature voicelessness. If there exists elsewhere in the language a phoneme /p/ in which this feature is relevant, it is neutralized in the combination /vp.../. The other phoneme in the example, /v/, may have the relevant feature voice, but this is not necessary. It is very well possible that a speech sound [b], in which we at the outset detected a feature voice, has to be broken up into two phonemes /mp/ in neither of which this feature is relevant.

As a rule, two *opposite* features belong to two different phonemes. However, there seem to be exceptions. A sound [ts] may have as distinctive qualities both interceptedness (compared with [s]) and continuousness (compared with [t]). This is a special case where +A of the above formula is the opposite of +B, or: +A = -B. Our analysis depends on whether a form with the artificial cluster /+A,-B,.../ /-A,+B,.../ is interpreted as identical with the form under study. This applies of course to all cases where two opposite features are contiguous, such as nasal and nonnasal. It cannot be excluded a priori that in some language a trinary dimension will be discovered of the type /n - nd - d/. The more conspicuous cases, such as /ts/ or /c/, are solved by Trager and Martinet²⁷ practically in the way I propose to do it with all phonemes. Only when [ts] differs functionally from [c] we can accept the existence of this latter as a phoneme; I prefer to consider it the

²⁷ See above, note 21; Martinet "Un ou deux phonèmes"...., pp. 97f.

intermediate term in the dimension running from intercepted to continuous; the dimension is linguistically parceled out as: stop – gliding from stop to spirant – spirant. It is also possible that we start from the supposition that [ts] is characterized by the feature “medium degree of local friction”, as compared with “minimal” in [s] and “maximal” in [t], for we are entirely free to select the point of departure. Then we are obliged, as in all cases where a medium portion of a dimension appears as relevant, to examine whether medium can be analyzed into positive plus negative (or negative plus positive, as the case may be).

It need hardly be emphasized that speaking of functional difference, I mean that the criterion of discreteness is also taken into account.

Finally we must reckon with the possibility that a phoneme occurs in the chain in immediate contact with an identical phoneme. Therefore we have to check whether a feature (theoretically any feature) can be interpreted as a repetition of the preceding or following phoneme. In practice only a few features will occasionally lead to such an interpretation, e.g. length, tensivity.

Here it is interesting to compare Jones' view: “If a given long vowel is separable into two, and if therefore in precise speech it could be pronounced double, it is convenient to regard it always as double.”²⁸ “Whether a vowel heard as long is to be considered double is a matter to be decided by the feeling of the *speaker*, which in its turn is determined by reference to the structure of the language”, for “although the difference between doubled vowels and single long vowels may often be inappreciable to the ear, and may be even objectively non-existent to listeners, yet the difference would seem to be in most cases subjectively perceptible to the speaker”. In my opinion, if a sound quality does not reach the hearer, it can scarcely play a role in a medium for communication. It seems harder to verify Jones' contention that “it is not possible to pronounce the French *mâitre* as [mɛ-etr]”, than to find a reliable answer to the question if a Frenchman will interpret [mæstr] as

²⁸ Jones, *The phoneme* . . . , p. 116.

maitre. The latter criterion seems also safer than the one employed by Hockett: "To treat Fijian as having 'vowel length' [instead of double vowels] is to deal with the facts very inefficiently",²⁹ for instead of striving after efficiency in the description, we should try to register the distinctive properties of the words and nothing but that.

To sum up: when two distinctive features A and B appear contiguously in a given form, and there can be found a realization of this form in which A appears definitely without B, let this form be real or artificial (no matter what other speech sounds are added), then A and B belong to different phonemes.

4. The procedure is rather complicated, for it has to be repeated for all contiguous features of a given sound complex. So in the short English word *bee* we find at least eight relevant features: (1) buccal local friction (as compared with absence of local friction resulting in a form without the original initial consonant, or in *he*, or else occurring *ceteris paribus* in combination with nasal resonance, as in *me*, or with cavity friction, as in *we*), (2) total oral occlusion (as compared with a lesser degree of closure, as in *vee*), (3) labial articulation (: *Dee*, *gee[se]*), (4) voice (and/or laxness; ³⁰ compare *pea*), (5) cavity friction (as compared with absence of cavity friction, occurring in *ba[lloon]* or in combination with some other feature, such as laterality in *bl[ack]*), (6) high tongue position (: *bay*, *baa*), (7) front + unrounded articulation (: *boo*), (8) length + tensity (: *be[come]*).

The relevance of the simultaneity of (5) and (6) need not be verified experimentally, since it is physiologically impossible to produce a sound with (6) which does not have at the same time (5). These features have been separately mentioned for the sake of symmetry, (1) and (2) being also taken apart. In both couples we have the positive term of one of the disparate dimensions treated on p. 35 above and the degree of closure responsible for it. In the case of (1) and (2) the two features cannot be listed under one

²⁹ Hockett, *Manual*...., p. 55.

³⁰ Jakobson, Fant & Halle, *Preliminaries*...., pp. 4f.

item, for (2) can be part of a sound which is not characterized by (1), to wit [m]. Nonmaximal local friction (as in [v]), however, can be listed as a single feature.

As we do not wish to accept in other cases phonetic simultaneity as conclusive evidence for grouping the features together into phonemes, we must assume that any two of them are separated by a phoneme border unless such a decomposition makes the word unrecognizable. Thus we are obliged to perform 21 experiments. This number cannot be reduced in the course of the procedure, for when we have demonstrated that voiced and occlusive cannot be assigned to separate phonemes, there still remains the possibility that they are jointly spread over two contiguous phonemes, and also that one of them is extended over more phonemes than the other. Thus in an initial cluster *st-* dentality can be separated neither from the spirant nor from the stop, but is at the same time coextensive with neither of them.

We start the syntagmatic analysis of *bee* with features (1) and (5). These are physiologically combinable. If (1) is immediately followed in the chain by (5), there will be some kind of a phonetic glide. When we try to pronounce *ceteris paribus* /+1,-5,.../ /-1,+5,.../ in order to check the relevancy of a supposed phoneme border, we get an artificial form, because the glide is omitted. This artificial form is phonetically characterized by so-called open juncture. In this realization the word retains its identity, and so the phoneme border is established. The preliminary outcome is (1) / (5). If the resulting form would be interpreted as different from the original one, the first phoneme would be /+1,+5,.../, and we would be forced to list a palatalized stop /b'/.

Features (1) and (2) cannot be separated, for a sound combining local friction and incomplete closure [v] preceded or followed by a sound containing no local friction, but complete closure [m], does not form the initial segment of a recognizable realization of *bee*. In the case of English it seems strange to make such a long detour to arrive at this obvious conclusion, but the method is meant to be generally applicable, and, for a language like Kikuyu, we learn

from the same experiment that initial [b-] is indeed to be phonemized as /mβ/.³¹

The other experiments lead to establishing one phoneme border: (1), (2), (3), (4) / (5), (6), (7), (8). Perhaps (8) admits a biphonematic interpretation, so that we get /bii/ after having ascertained that (5), (6), (7) are necessarily present in both vowels. Another possible solution is /biy/, defensible when, instead of (8), presence versus absence of a final nonsonorous segment differentiates *bee* from *be[come]*. It is also possible, and even more probable, that the opposition sonorous : nonsonorous is neutralized in this position. It should be emphasized, however, that the selection of one of these possibilities is not a question of predilection on the basis of efficiency, symmetry or the like. Neither should it depend on the type of procedure. The only decisive factor is the response of a well-prepared native speaker to the respective sound complexes.

The unity of all phonemes can be established on the basis of these principles. The criterion is essentially the same as the one that ascertains the relevant features, to wit the distinguishing power, which we test by investigating a phonetic dimension. In the case of mono- or biphonematic interpretation, this dimension runs from simultaneity, [p^o], through close juncture, [ph], to open juncture, [p-h]. At the start, these distinctions are purely phonetic; we have a complete control of them, since they form the input for the experiment. If it turns out that the dimension is lotted out functionally (in which case the border point between the relevant areas will be situated either somewhere between open juncture and close juncture, or between close juncture and phonetic simultaneity, according as the outcome of the experiment may be), we have established a paradigmatic opposition between a number of distinctive features belonging to one phoneme on the one hand, and the same features distributed on both sides of a phoneme border on the other. If the dimension is not exploited for communicative ends, the simultaneity of the features has no relevancy, i.e. they are functionally not simultaneous.

³¹ See Jones, *The Phoneme* . . . , p. 208.

One restriction must be made here. If a number of features distributed over two neighboring sounds function differently from the same features combined into one sound, we have yet no right to interpret the latter as one phoneme until we are sure that the phoneme border in the former arrangement is not always at the same time a morpheme border: an example has been treated already in the preceding chapter (p. 57): Polish [ts] and [c] are discretely relevant, but since [ts] contains always a morpheme border, both are to be regarded as clusters of /t/ and /s/.

As a consequence of this approach – and I do not consider it a regrettable consequence – the phenomenon of juncture disappears from the list of possible distinctive qualities, for open juncture will simply be a realization either of a phoneme border or of a morpheme border. Once we have ascertained significant boundaries that cut the sound chain into segments, a distinction of different types of phoneme boundaries is linguistically unmotivated. An extreme example contrary to this contention seems to be Cantonese, about which Hockett remarks: "Writing a hyphen for syllable juncture, it is possible to get such contrasts as the following: /...an-a.../, /...a-na.../, /...a-n-a.../ – to say nothing of /...an-na.../, /...an-n-a.../, /...a-n-na.../ and the like".³² The procedure of the present study – provided of course that the hyphens do not stand at the same time for morpheme borders – leads to the assumption of prenasalized and postnasalized vowels, /^{na}/ and /^{aⁿ}/. To be sure, these are rather exceptional phonemes, but Hockett's system with its high frequency of open junctures is no less surprising. The only objection to these complex phonemes can be inspired by phonetic considerations: two mutually exclusive features occur jointly in one phoneme. But the same objection can be directed against all affricates. Hockett assumes a phoneme /č/ for Polish (p. 100), although the difference between this phoneme and the cluster /tš/ is evidently a question of juncture. This appears clearly from his definition of "affricate": "a close knit stop-spirant sequence sharing a position of articulation" (p. 40).

³² Hockett, *Manual*..., p. 60.

Hockett himself expects to find opponents who will be disturbed by the introduction of juncture: "But why should we attempt to discover and recognize any such peculiar elements in a phonologic system? Why cannot one set of principles be used throughout? [...] junctures indeed stand out of line, and it is a fact that a phonologic system can be completely analyzed without recognizing any junctures at all. The contrast between *nitrate* and *night-rate* can be provided for by setting up two different [r]-like phonemes; that between *shyness* and *minus* by setting up /ay/ and, say /ay·/, as different units, perhaps subsequently extracting the 'drawl-feature' /·/ as a lower-level unit" (pp. 168-9). If we forget for a moment that the contrasts mentioned in this passage are fully accounted for by the morpheme borders, we cannot but grant the possibility of the offered solutions. Perhaps they will fit also the Cantonese material, so that we may interpret the hyphen in the above phonemizations as a separate drawl-phoneme, or as a feature of the preceding phoneme. A successive application of the criteria for paradigmatic and syntagmatic delimitation will instruct us whether a drawl-phoneme /-/ or phonemes with significant duration /a·/, /n·/, or finally phonemes with nasalization /aⁿ/, /ⁿa/ are prescribed by the system.

Hockett rejects all these solutions because he gives preference to an analysis into immediate constituents (ICs). This is the crucial point of his theory. He criticizes in this respect the most important schools of phonemics: "Now what the Americanist and Praguian traditions have in common is an insistence that vertical cutting [transverse to the time axis] be completed before any other kind of cutting into ICs be attempted, and that once one has shifted to any sort of 'decomposition' of segmental units into simultaneous components, no more vertical cuts be allowed. In our discussion of IC-procedure we made no such blanket rule, and, indeed, did otherwise in a number of examples. One of the examples was that of clusters of obstruents in English. We broke such codas [final clusters] as /ts dz ps bz st zd pt bd/ first into ICs /TS PS ST PT/ (voicing-irrelevant constituents) and /H/ or /A/ (voicelessness and voicing). Only after this did we proceed to break /TS/, by a vertical

cut, into a voicing-irrelevant apical stop /T/ and a voicing-irrelevant spirant /S/, in that order" (p. 165). I agree with Hockett that it does not seem to make such sense to complete the vertical cutting before the distinctive features are ascertained, and, stronger, I believe that the syntagmatic delimitation cannot have a start until the paradigmatic analysis is finished. Hockett does not go that far: he analyzes *cats* first into the ICs *c* and *ats*, for "examining English syllables in general, we find that while a great variety of microsegment-initial syllables occur with a peak not preceded by an onset, only certain special types of microsegment-final syllables have a peak not followed by a coda" (p. 150), then he breaks *ats* into *a* and *ts*, and only thereupon he breaks *ts* into voice-irrelevant /TS/ and voicelessness, instead of cutting it into *t* and *s*. The last solution is somewhat surprising, for a group /TS/ with neither voicing nor voicelessness occurs nowhere in the language, whereas *t* without *s* and *s* without *t* are frequent. So we notice a shift of criteria. It is worth while to see what the analysis of *-ts* would have been like, if Hockett had been consistent. We assume for a moment, as Hockett does, that the three constituents to start from are voice-irrelevant /T/, voice-irrelevant /S/ and voicelessness /H/. Hockett's principles of IC-analysis no doubt dictate a different solution from the two envisaged a moment ago. They rather require for /H/ the role of a "shared constituent" belonging to both ICs at the same time. In this way Hockett describes the position of /l/ in *believe*: "The breakdown can be indicated as follows: First, into (bĩ(l)/ and /l(i)yv/, where the parentheses indicate that there is just one /l/, but that it has to be assigned to both ICs" (p. 152). It is obvious that, following the same line of argument, we would have to analyze *-ts* into the ICs /T(H)/ and /(H)S/. It is very instructive to trace the origin of Hockett's inconsistency. It is to be looked for in the fundamental difference between a horizontal (along the time axis) and a vertical arrangement of the items. As a consequence of the linear, sequential character of all linguistic utterances, horizontally arranged items are additive entities (*l* in *believe*), while vertically arranged items are properties (such as voicelessness in *-ts*). A further practical consequence is that in the analysis we are at a loss

whether we shall write /*(H)S*/ or /*S(H)*/, whereas a similar problem does not exist in the case of /*(l)iyv*/. Breaking up an object into parts and abstracting qualities from an object are incomparable operations: they can hardly be performed on the basis of the same set of principles without running into difficulties.

I wish to stress once again that it is not advisable to start the analysis with a preconceived idea about the status of the relevant elements, i.e. which of them are properties and which additive entities. We first seek to discover only distinctive properties, but no matter which further way of analysis we choose, we will always some time encounter those groups of features for which the mutual arrangement is irrelevant and, for that reason, not specifiable. Hockett's IC-analysis, too, will be faced with them. These groups will always clearly stand out. There seems to be no reason why we should not call them "phonemes".

It is the aim of this chapter to show that the abstraction of an element called "phoneme" is prescribed by the language itself. It is not an invention of linguists, but an inherent part of every language system. Thus I consider indefensible the widespread opinion that different linguists may set up different phoneme inventories for the same language according as they choose to define the phoneme. This is precisely Hockett's point: "[...] there are too many chances for alternative interpretations of phonologic systems" (p. 138). Elsewhere the same author writes: "The phraseology [phoneme versus cluster] implies that a [č]-type phone, for example, must either be a unit phoneme or a cluster of an integral number (presumably two) unit phonemes. By any one precise set of rules for the use of the term 'phoneme', this is perhaps true, but this simply shifts the arbitrariness to the rules instead of to their application" (p. 161); "[...] the question 'is /č/ a phoneme or a cluster?' is too narrow and not properly fruitful; a more productive question to ask is 'on what hierarchic level, or levels, does the given element function, and at what step, working down the IC-scale, does it break down into smaller constituents?' [...] the term 'phoneme', and just how to use it, is regarded as of subsidiary importance" (p. 164). But, "working down the IC-scale", the

investigator will reach a point where he has to decide how to isolate ICs in a group of two phonemes, and then the analysis by a vertical cut will impose itself, for a phoneme has always a less restricted distribution in its full shape than in the reduced shape it assumes after one feature has been substracted (than a so-called "archiphoneme"). Therefore I venture a rule: A constitute consisting of two phonemes has these phonemes as ICs. I refer in this connection to the IC-analysis of meaningful forms: Wells, to whom we owe the development of the IC theory, formulates "the principle that word divisions should be respected" (p. 84).³³ This holds true *mutatis mutandis* also for phoneme boundaries.

The most important difference between IC-analysis and phoneme analysis is that the latter is interested only in *significant* borderlines: an IC boundary which is not at the same time a phoneme boundary, is not a potential distinctive feature of the larger unit decomposed by it. Thus the line cutting the /l/ into two halves does not distinguish the word *believe* from an otherwise identical word without it; if this were the case, we would have to assume a double phoneme /ll/ for *believe*. The units obtained through the IC-analysis are often of a type that would fail to turn up in a description concerned with distinctive properties only. This applies in the first place to the syllable, the most fundamental unit in Hockett's technique. For this reason, a phonologic analysis limited to isolating phonemes is less incomplete than a mere analysis into ICs, because it reflects everything that equips language as an instrument for expressing meaning.

As a matter of fact, the problem of mono- or biphonematic interpretation is not meaningless to Hockett, despite his categorical statement quoted earlier. He even offers a criterion for solving it: "The cluster interpretation is impossible if there is no appropriate element in the system which can function as the 'modifying' element of each cluster. Thus a [p'] can be a cluster provided there is also a [b] or [p] and an independently occurring /h/, but if there

³³ R. S. Wells, "Immediate Constituents", *Language*, XXIII (1947), pp. 81-117.

is no independent /h/ then an interpretation as /ph/ is precluded" (p. 124). I doubt the validity of this criterion, for I do not see why a limited distribution of /h/ should be unacceptable. When we want to know whether Dutch [ha] is phonemically an aspirated vowel or a combination of two phonemes, we certainly do not let our interpretation be influenced by the fact that there is no independently occurring /h/.

I hope to have shown in this chapter that a syntagmatic delimitation of the phonemes, no less than their paradigmatic delimitation, rests on the principle of distinctiveness, the most important principle in linguistics. Consequently, the phoneme is an indispensable unit in any linguistic description.

The importance of a well-defined procedure of segmentation appears from a passage quoted from Miss Fischer-Jørgensen³⁴: "In Hungarian there are 3 nasal phonemes: /m/, /n/, and (palatal) /ɲ/, which have been described by John Lotz (in a private letter) in the following way: 'The allophones of /n/ occur in the following mutually exclusive environments: a velar [ɲ] before *k* and *g*, and a dental [n] in all other positions. [ɲ] can only be assigned to the /n/-phoneme, since before *k* and *g* it contrasts with both [m] and [ɲ]. Neither arrangement of these nasals in a linear series according to place of articulation nor a scheme of two binary oppositions grave-acute, compact-diffuse is logically feasible, since the /n/-phoneme cannot be characterized by any exclusive feature.' It seems impossible to find a satisfactory solution to this problem. But there are the following possibilities: (1) *n* and *ɲ* are considered as separate phonemes. From a functional point of view this is not a good solution. (2) Different features are used in different surroundings. (3) The description mentioned at the end of the paragraph on segmentation might be applied, i.e. the velar feature of *ɲ* is part of the *k*-phoneme." This passage calls forth two remarks: (1) There is no problem at all as long as it has not been proved that the interpretation of [ɲ] as a cluster of phonemes /nɲ/ is wrong. (2) If we accept for a moment the monophonematic interpretation

³⁴ Fischer-Jørgensen, "What can the New Techniques....", p. 475.

of the palatal sound, Miss Fischer-Jørgensen's third solution is the right one, for the fact that a Hungarian interprets an artificial [mink] as a strange realization of *mink* "we" (usually pronounced as [mɪŋk]) indicates that the features of nasality and velarity are separated by a phonemic borderline. Not velarity is then a distinctive quality of the nasal, but lack of palatalization.

III

PARADIGMATIC DELIMITATION OF SEMANTIC UNITS

1. Linguists approach language usually from the side of the form. It is certainly possible to establish phonemic systems without having recourse to meaning at all. In fact, I have tried to do so in the present study. To be sure, the conclusions were based upon the judgment of the natives, and it cannot be denied that this derives ultimately from the experience that sounds stand for something else, but the mere habit to employ in general sound features as distinguishing marks is quite sufficient to account for the fact that sounds "feel" different or same; any human being, accustomed to work with sound properties as instruments, is ready to recognize them also when no otherwise identical forms with the opposite features can be found (whether he really does so in every single case is a question of experiment).

Furthermore, Bloch has attempted to ascertain phonemes without any information that can be said to be even indirectly connected with meaning.¹

But on the other hand, it is in advance precluded to describe the semantic system of a language while leaving the form out of the picture. Most generally accepted methods of analysis start from phonetic differences, such as the one between [d] and [t], and look for pairs of words where they differentiate meanings, but we know of no scholar who asks if a given language expresses differently a wish and an incentive, or "liquid" and "water", or "horse" and "stallion", and who tries to arrive in this manner at a complete inventory of the relevant semantic features.

This state of affairs is fully explained by Mrs. Langer's question:

¹ B. Bloch, "A Set of Postulates for Phonemic Analysis", *Language*, XXIV (1948), pp. 3-46.

"What is the difference between a sign and its object, by virtue of which they are not interchangeable?" and her answer to it: "The difference is, that the subject for which they constitute a pair must *find one more interesting than the other, and the latter more easily available than the former.*"²

The fact that the form is more easily accessible may be illustrated by an example. If we ask a person whether a word we pronounce contains the *p* of *peak* or the *b* of *beak*, he will be able to answer that it is the one or the other, or something in between. But when we show him a stallion and ask if it is called *stallion* or *horse*, he will be at a complete loss, unless he answers: "Both". This is a very simple instance of how two meanings may overlap. When we ask: "Is this a child or a kid?" a difference of meaning is involved which does not show on the presented object, and which we may call "the attitude of the speaker". These two types of complications compel us to start, for both phonemic and semantic inquiries, from the word *form*.

This does not imply, however, that meaning is a kind of second-rate constituent of the language system, which is not worth while to pay attention to, or that it is impossible to gain any information about it. On the contrary, I believe we can analyze meaning as completely as form. I cannot accept the widespread view that language structure (and, consequently, the structure of the science of language) is necessarily and fundamentally asymmetrical, that it consists of two parts, sound and meaning, the former of which can be further analyzed than the latter.

Whole schools of linguists – I ought to mention here in the first place the Bloomfieldians – do not feel very happy when they have to face problems of meaning. The fact is overstressed that there is no linguistic meaning without a corresponding expression; direct data on meaning are often considered subjective, and thus unreliable.

The idea of asymmetry is maintained by an author who is otherwise not afraid to tackle problems of meaning, Ullmann.³

² S. K. Langer, *Philosophy in a New Key. A Study in the Symbolism of Reason, Rite, and Art* (1942), Chapter 3.

³ St. Ullmann, *The Principles of Semantics* (Glasgow, 1951), pp. 36, 39.

We read about the structure of language: "A combination of the twin criteria of symbolic function and structure yields the following interim arrangement, characteristic in its asymmetry:

- I. Phonology
- II. Lexicology: (1) lexical morphology;
(2) lexical semantics.
- III. Syntax: (1) syntactic morphology;
(2) syntactic semantics."

In this diagram the distinction between a science of the form and a science of the meaning is restricted to lexicology and syntax; in the orbit of phonology only the former is present. Let us not be misled by terminology: the one employed here already excludes at the start complete symmetry, since "phonemic semantics" would be a *contradictio in terminis*. But if we replace "phonology" by, say, "nuclear morphology", we could restore the symmetry by adding "nuclear semantics".

Ullmann writes: "Language can only be reached through speech, and the complex structure of the latter prescribes two convergent routes of analysis. To take the physical route first, connected discourse can be decomposed into acoustic portions of varying sizes, then into syllables, and finally into sounds" (p. 29). "Secondly, connected discourse may be analysed from the *semantic* point of view. It will then fall into a certain number of meaningful segments which are ultimately composed of meaningful units. These meaningful units are termed *words*" (p. 30).

Here Ullmann is in accordance with Bloomfield (only the latter goes one step further and takes as the end of the semantic analysis the morpheme instead of the word, but this difference does not occupy us here). Bloomfield writes: "A morpheme can be described phonetically, since it consists of one or more phonemes, but its meaning cannot be analyzed within the scope of our science. For instance, we have seen that the morpheme *pin* bears a phonetic resemblance to other morphemes, such as *pig*, *pen*, *tin*, *ten*, and, on the basis of these resemblances, can be analyzed and described in terms of three phonemes [...] but, since these resemblances are not connected with resemblances of meaning, we cannot attri-

bute any meaning to the phonemes and cannot, within the scope of our science, analyze the meaning of the morpheme.”⁴ He further reasons: “A workable system of signals, such as a language, can contain only a small number of signaling-units, but the things signaled about – in our case the entire content of the practical world – may be infinitely varied. Accordingly, the signals (linguistic forms, with morphemes as the smallest signals) consist of different combinations of the signaling-units (phonemes), and each such combination is arbitrarily assigned to some feature of the practical world (sememe). The signals can be analyzed, but not the things signaled about. This re-enforces the principle that linguistic study must always start from the phonetic form and not from the meaning” (p. 162).

We see that Bloomfield’s chief argument rests on the fact that the phonemes have no meaning of their own. In other words, he decides that a semantic analysis of the morpheme is impossible after he has tried in vain to subject the *form* to a *semantic* analysis. This is, in my opinion, an attempt foredoomed to failure, precisely as if someone would seek to analyze the meaning into phonemes. In this respect Ullmann arrives at the same conclusion as Bloomfield, and along the same way.

Bloomfield says: “There is nothing in the structure of morphemes like *wolf*, *fox* and *dog* to tell us the relation between their meanings; this is a problem for the zoölogist” (p. 162). Here “structure” obviously points to the structure of sound complexes. If we comprise in the “structure of the morpheme” also its semantic make-up, we may easily detect a common semantic constituent in words like *horse*, *stallion* and *colt*, without leaving the field of linguistics proper.

As a matter of fact, we know from a number of studies, among which those by Jakobson are perhaps the most important, that a semantic analysis of morphemes is very well possible, provided that it is the content we decompose into underlying semantic entities, and not the sound. I just quote one passage from Jakobson:

⁴ L. Bloomfield, *Language* (London, 1935), pp. 161f.

"The Latin inflectional affix *-mus* is commutable with a set of other affixes and hence carries the meaning of the first person as opposed to *-tis*, the meaning of the plural in opposition to *-o*, that of the active against *-mur*, etc. Consequently the conceptual counterpart of the formal unit *-mus* is a bundle (Bally's *cumul*) of *semantic minimums*."⁵

Ullmann argues: "Phonemes do not lend themselves to the distinction between form and meaning" (p. 31). Other symbols, labeled by Ullmann "full symbols", have, "in the Saussurean terminology, an external facet, the 'signifiant', and a semantic facet, the 'signifié'." Phonemes are "diacritical marks distinguishing words from each other. They can therefore have no 'signifié' of their own". "At this point the internal structure of language reveals a curious lack of symmetry."

Here Ullmann forgets that Jakobson's "semantic minimums" do not lend themselves to the distinction between form and meaning either; that they, too, may be considered diacritical marks distinguishing words from each other; finally that they do not have a "signifiant" of their own either. So the symmetry is again restored.

Thus I assume a basic symmetry in the system of linguistic entities. On both sides of the line that divides the two planes of sound and meaning we find elements having a one to one correspondence with elements on the other side (word-forms and word-meanings) and elements without such correspondence (phonemic and semantic minimums). Ullmann's contention that "meaning enters discourse at a higher level than sound" (p. 32) is obviously a fallacy, unless we understand "discourse" here as "the sounds of discourse".

In Ullmann's parallel "If phonemic substitution results in different word-meanings [...] lexical substitution yields different phrase meanings" (p. 55) I would prefer to read instead of the latter half: "semantic substitution yields different word-forms". Then we would have in both halves a crossing of the line separating sound and meaning.

⁵ R. Jakobson, "The Phonemic and Grammatical Aspects of Language in Their Interrelations", *Actes du Sixième Congrès International des Linguistes. Rapports* (Paris, 1948), pp. 4-18 (p. 7).

Nevertheless Bloomfield's statement remains true: "Sometimes we may be unable to decide whether phonetically like forms are identical in meaning [...] This difficulty is part of the universal difficulty of semantics: the practical world is not a world of clear-cut distinctions" (p. 160). This obstacle, however, is not as insurmountable as it appeared to Bloomfield. We must not forget that speech sounds also form a part of the same practical world, and in this respect compose a sensory field that is not parceled out as long as we study it from a physical standpoint. But this continuum is lotted out by each language in a different way in order to make it suitable to be employed as a medium for communication. In the same way meaning presents itself to us as a amorphous body of immediately apprehended sense data, which we may call the "semantic continuum". The languages of the world, each of them in his own way, mold this unorganized mass into a definite shape, required for the linguistic manipulation of it, for its use as the subject matter of communication. Therefore I believe that Hjelm-slev's quadripartition of linguistic phenomena is essentially correct: substance of expression : form of expression = substance of content : form of content.

The semantic continuum cannot be anything else but the real world, accessible to our senses. Parts of it, when referred to by means of a linguistic form, are usually called "things meant". The meaning of a linguistic form (morpheme or word) is its ability to point to (to denote) a number of things in reality. It is not to be identified with the "things meant", but it certainly can be defined in terms of them.

Investigations in this field are not very numerous, but the possibility of such a semantic analysis finds a widespread acceptance. Even Bloomfield himself admits it to a certain degree: "Although the linguist cannot define meanings, but must appeal for this to students of other sciences or to common knowledge, yet in many cases, having obtained definitions for some forms, he can define the meanings of other forms in terms of these first ones [...] If the difference *male* : *female* is defined for the linguist, he can assure us that this is the difference between *he* : *she*, *lion* :

lioness, gander : goose, ram : ewe" (pp. 145f). On the beginning of this passage we may again comment that the linguist cannot define forms, either, without having recourse to auxiliary sciences (physics, physiology) or common knowledge. In this respect there is no difference between form and meaning.

Here it is necessary to mention a well-known fact which imposes certain restrictions on this treatment of meaning. It will be presented here in a simplified form, for only its linguistic implications are envisaged. When we establish the meaning of a word by collecting and observing the realities to which it points, we find inevitably cases where no pointing to a concrete situation takes place. A sentence like *A cow is an animal* points to reality in general, and consequently does not yield in this way any information on the meaning of the word *cow*. Nevertheless it is precisely the ability of the word *cow* to point to certain things in reality which lends sense to this sentence. As a matter of fact, the information which forms the content of the sentence cannot be utilized for linguistic purposes, although such sentences are often employed for communicating information previously gained through a linguistic inquiry.

On the other hand we frequently find that a meaning is meant to be applied to a reality to which it is not literally applicable. In certain cases we speak of a "lie", for example when someone says: "A cow has bitten me", while actually it was a dog. Obviously the sentence does not fit the reality for which it is intended. A linguist is not interested in the problem of truth, but if he wants to ascertain the meaning of *cow*, he certainly must take care to consider only the realities to which a sentence is adapted. He can learn this only from a native. He must never forget that a linguistic form can be made to point *indirectly* to realities to which it can not point directly, by virtue of its meaning. In the case of a lie, the indirect pointing is caused by extra-linguistic factors. But there are also cases where the indirect pointing is linguistically conditioned, to wit, by the position the meaning of the form occupies within the linguistic whole. Thus, in the line of poetry *The moon was a cow*, the syntactic construction is *inter alia* characterized by the

fact that *cow* points to the same thing in reality as *moon*, or, in other words, *cow* points only indirectly to reality. The same can be said of *cow* in an exclamation like *You cow!* In such examples the syntactic construction prevails for the selection of the reality to which a meaning is applied, and the meaning itself does not play a role in this respect. Consequently, the range of applicability of these sentences does not help us in determining the applicability of *cow*. When we pay attention to these restrictions, the "things meant" indeed furnish all the semantic information a linguist needs, in the same way as they are the source from which a child learning its language acquires most of the meanings.

Thus it is justified to reduce the alleged basic asymmetry of language to a difference of approach to its phonetic and semantic aspects, which is a consequence of the fact that the former is more easily available. As an example we take the word *wood*. It consists of a form and a meaning. The phonemic analysis has to start from what is immediately given: a sequence of speech sounds pronounced at a certain moment. This realization of the form may be linguistically identified with other realizations of the same form, and distinguished from realizations of other forms, by the operation indicated in Chapter I. For instance, a lengthening of the vowel results in a different word: *wooded*. If we should start the semantic analysis, however, from a thing meant at a certain moment, we would not get very far. The thing meant is a realization of the semantic form "wood", but it is very well possible that a repeated change of the thing along the lines of various properties (in the same way as I have proposed to shift the sounds along the separate dimensions of the phonetic continuum) will never lead us to a thing for which the denomination *forest* is obligatory instead of *wood*. The reason is, as already remarked above, that a meaning is not wholly determined by the thing meant, but rather by the sum of the thing meant and the speaker's attitude towards it. Therefore, a relevant semantic feature can be ascertained on the basis of the existence of two different word forms (or morpheme forms). In other words, the phonemic difference prevails.

This means that English /ruwt/ and /rut/ (both written *root*),

since their formal features mark them as two different words, have different meanings, too; because they occur in the same positions in the utterance, the formal variation is certainly not conditioned by the environment. An attempt at a semantic identification would fall outside the scope of linguistics, and a linguist can only resignedly declare: "Perhaps ultimately the meanings of the two forms [...] are slightly different."⁶

Once we have established relevant semantic differences, the only thing we can try is to describe them after having compared all of the things to which the meanings they set apart can be applied. Linguistically speaking, however, we do not come in this way a single step farther than when we simply list the features in the shape of proportions, such as *wood* (: *forest*), *wood* (: *garden*), *wood* (: *timber*), *wood* (: *tree*), *wood* (: *metal*), etc.

It is even questionable whether such a list of distinctive features of meaning belongs to the system. They differ fundamentally from the distinctive features of sound. They do not "call univocally, reversibly and necessarily for a definite opposite, while any distinctive [sound] feature does" (Jakobson, see above p. 25). If we subtract the feature "*wood* (: *forest*)" from the meaning "wood", we do not find an element shared by both "wood" and "forest" (as /p/ and /t/ share the element voiceless stop). The meaning "wood" is distinguished from the meaning "forest" by its whole make-up. In a nonlinguistic sense there may be a common denominator, but this cannot be ascertained linguistically. Consequently, detecting the features does not lead to analyzing elements of meaning. As a further consequence, the meaning of *wood* cannot be exhaustively described by enumerating whatever number of these features, for the number of elements which can appear in the same positions (in the semantic chain) as "wood" is infinite. It can be easily increased: in the case of *wood* we may say that the invention of plastic (and of the matching word) has added one item to the number of distinctive features. Moreover, a chemist will find many more distinctive features in "water" than an ordinary individual.

⁶ Hockett, *Manual* . . . , p. 146. See further Chapter V of this study.

For these reasons, while the distinctive sound feature is a workable phonemic minimum, the distinctive feature of meaning cannot serve as a semantic minimum. It is below the level of linguistics proper. Almost everything said in the preceding paragraph about the feature "wood (: forest)" applies to the phonetic difference between, e.g., voicing and total occlusion (with the one exception that these phonemic features cannot replace each other).

Semantic minimums are the ultimate semantic constituents which are separately interchangeable in the positions where they occur. Only this definition warrants the linguistic relevance of the analysis. In this sense, the semantic minimums are on a par with the distinctive features in phonemics. This equality as to rank can be maintained, of course, only with respect to the patterning of the two linguistic planes. Objectively speaking, however, we work farther down on the plane of the form.

Only when a position in the semantic chain admits a selection from an integral number of elements, is the semantic minimum coextensive with the (nonlinguistic) distinctive feature of meaning. For example, in a Russian sentence the element "perfective" can only be replaced by "imperfective". In this case we can distinguish within the semantic continuum a dimension "perfective – imperfective", while other semantic minimums, such as "wood", occupy disparate areas.

Ultimate constituents, both of form and meaning, are as a rule not objectively, physically, simple. In one of Martinet's articles the term "voice" "designates an articulatory complex that is not dissociable into simpler distinctive features".⁷ This applies also to the semantic minimums, even when they are located along a dimension, for it happens that a group of dimensions in reality is evaluated by a language as one functional dimension. An example is again the Russian dimension "perfective – imperfective". A perfective verb indicates that the event is single, complete and nonsimultaneous with the moment of observation, whereas an

⁷ Martinet, "Phonetics and Linguistic Evolution"...., p. 256.

event expressed by an imperfective verb is either repeated, or incomplete, or observed at a simultaneous moment, or characterized by any combination of these features.⁸ This picture shows the same complexity as, for instance, the one presented by the feature setting apart the phonemes /a/ and /ə/ in Dutch (see above, p. 31).

2. We come next to the procedure for recognizing semantic minimums.

The examples of semantic elements adduced thus far are paradigmatically delimited by virtue of their phonemic distinctness. They derive their discreteness from the plane of the sounds. This does not imply, however, that identity of form is always an indication of identity of meaning. When we have to deal with possible homonyms, the reference to the form fails. In that case our only data are the amorphous mass of meaning and the judgment of the native, who may or may not draw borderlines in this "substance". Of course, we cannot expect a reliable answer if we simply ask an informant: "Are *pair* and *pear* different words or appearances of one word?"

I propose the following experiment. We confront the informant with a spoken sentence where two applications of a given word are equally admissible. This is a necessary condition, for always when one of the meanings of the word is excluded, this may be caused by the context, as in *he ate a pear*, where the choice of the meaning "fruit" may be dictated by the meaning of the verb, and consequently does not yield the required information about *pear*. So we start from an utterance like *he missed one pear/pair*, devoid of any context. The informant will be unable to decide which meaning is pertinent, but he is in a position to say whether he received the complete message the speaker wanted to entail. In the case of *pair/pear* this is certainly not the case. An extreme example of the opposite outcome would be *he bought a table*, with the alternate meanings of "round table" and "square table". The informant will again not know which of the two is meant, but here he will be sure that he got the full information aimed at by the

⁸ See my paper "On the Verbal Predicate in Russian", *For Roman Jakobson* (The Hague, 1956), pp. 83-90.

speaker, although he may think that this information is not specific enough. If he is aware of a failure, he has to attribute it to the speaker, and not, as in the preceding case, to the language (although, even in the case of a homonym, he may blame the speaker for not avoiding the disturbance caused by the deficiency of the language). However, I admit that this procedure does not always work, for it requires too subtle a distinction on the part of the informant.

Therefore it is necessary to find a secondary indication about the answer we cannot obtain directly. We assume that two possible applications of a form are linguistically distinct, until we come across an occurrence where the form of necessity points to a reality equally well equipped for both supposed meanings to appear as their actualization. Such an occurrence of *table* is: *Why isn't there a table in this room?* We have in this way a tool for checking functional distinctness of meaning. A less extreme example than the preceding ones is *game* as "act of playing" and "apparatus employed in playing". In reality, as we observe it with no view to linguistic expression, there scarcely exists a thing or state of affairs ready to be covered by both definitions. In reality as it is parceled out by the English language, however, such "things" do indeed turn up, as in the sentence: *The game takes up all of his time.* So we are forced to decide in favor of one linguistically homogeneous meaning, no matter how hard it sometimes may be to reproduce it by means of a single paraphrase.

For this operation we must take care to avoid puns. In the case of a pun, a form is used in such a way that the thing meant is capable of serving as the actualization of two linguistically distinct meanings expressed by homonymous forms. The pointing to such a thing may be possible, but is never obligatory: this fact furnishes a criterion for eliminating these cases. The same argument leads to the separation of those meanings of homonymous forms which stand to each other in a genus-species relationship.

The two definitions of *game* represent variants of one linguistic meaning; this means that the two forms have all their relevant semantic features in common.

The procedure indicated here is based upon the absence (or presence) of a clear-cut borderline between two applications of a word. In other words, it is in semantics, as in phonemics, the criterion of discreteness which directs our analysis. I am convinced that this is the point of departure which gives language its due, if we insist on language being a code.

A rather complicated case we will have to subject to the criterion is the one of *sheep*. The form may stand for one sheep or for a number of them. We come across occurrences where the thing meant fits both interpretations: *sheepshearer*, *sheep-like*. The semantic difference, however, recurs in forms where it has an unmistakable linguistic expression, such as *cow* : *cows*, and here the same number-irrespective use is found in analogous positions: *cowcatcher*, *cowbell*. This shows that the semantic opposition "singular : plural" is neutralized in certain positions, and it goes without saying that possibility of neutralization is not a proof of non-discreteness. Since a merger of the two meanings of *sheep*, wherever it occurs, can be explained as a neutralization, they must be considered linguistically different.

The operation can be performed only when the phonemically identical forms occupy the same position in the utterance. But there are three more cases to be envisaged: (2) phonemically different forms in the same position, (3) phonemically identical forms in different positions, (4) phonemically different forms in different positions.

On the plane of the meaning, a direct investigation of the substance as it is interpreted by a given language (i.e. by the speakers of that language when they handle the substance in an act of communication) is impossible, for reasons indicated earlier. Therefore the analysis must proceed in the opposite direction. Consequently, in a linguistically satisfactory way no semantic features can as a rule be ascertained which participate in the characterization of different forms, but do not exhaust their content. This is case (2). The semantic minimum "wood" cannot be broken down any further. The "wood"-feature resulting from the comparison "wood : forest" is really the end of the analysis. Nevertheless, there are other cases where a further analysis is feasible. For this purpose I

would like to define the semantic minimums as "recurrent distinctive features of meaning". Recurrences are best expressed in the shape of proportions, e.g. "stallion : mare = ram : ewe". Their validity can be established by means of an experiment: a foreigner supplied with the proportion and with information about the meaning of three of the terms must be able to infer the meaning of the fourth. This procedure will call forth the objection that the feature of meaning is even in such cases complex, or – what often amounts to the same thing – vague, a fact which may be illustrated by the example of a child distinguishing perfectly between a cow and a bull, but unaware that the one is a female and the other a male. So their difference is not exhausted by the reference to sex, but complicated by other characteristics, as, for instance in the case of the bull, stature or aggressiveness. When we examine this example more attentively, however, we soon learn that the feature of sex is decisive and the other features are to be regarded as "redundant". We know from phonemics that redundant features are under certain conditions ready to assume the role of distinctive features and to serve as their substitutes. This accounts for the right interpretation of the child.

Since this procedure concerns phonemically different forms, we must be sure that all requirements concerning position are met with. This point will be treated in the next chapter (see pp. 104ff).

The proportion "stallion : mare = ram : ewe" yields a pair of opposite semantic minimums, to wit the distinctive qualities common to both halves: "male : female". The same proportion can be restated as "stallion : ram = mare : ewe". So we obtain another pair of semantic minimums: "horse : sheep". The semantic minimum "horse" is the *tertium comparationis* of the comparison "stallion : mare". From a linguistic point of view, the comparison "wood : forest" does not lead to isolating a *tertium comparationis*. Nor does "male : female"; to be sure, this opposition recurs in "stallion : mare", but the common element equals zero.

The semantic minimums detected in this example find a separate expression in the linguistic forms *male*, *female*, *horse* and *sheep*. It should be noted, however, that this is not a necessary requirement:

there are also semantic minimums to which it does not apply. In many languages an element "genitive" occurs only in combination with "singular" or "plural". It is precisely the necessity of isolating such elements that forces us to employ the criterion of recurrence, although the analysis dictated by it may in other cases seem less remunerative.

There are linguistic forms the semantic aspect of which consists of a number of semantic minimums, e.g. *stallion*, while other ones contain only one semantic minimum, e.g. *good*.

Apparent synonyms also belong to case (2): they are phonemically different and have the same distribution. But only rarely a common semantic minimum can be detected in them. For English *root* (see above) we might be tempted to set up e.g. $/ruwt/ : /triy/ = /rut/ : /triy/$, but the impossibility of the restatement $/ruwt/ : /rut/ = /triy/ : /triy/$ shows that it is not valid. Consequently the two forms *root* do not have the same semantic make-up. (See further below pp. 127.)

In the examples of case (3), phonemically identical forms in different positions, the possibility of setting up a proportion also allows us to ascertain a common semantic minimum, e.g. in the substantive and the verb *play* on the basis of $(the) play : (the) work = (I) play : (I) work$. The proportion shows that the phonemic sameness of these forms occurring in different environments is not fortuitous. This type of proportion is less rewarding than the one of case (2), for we do not know beforehand where the borderlines are to be drawn between the meanings of the forms themselves and those of their environments. So the only thing we can say is that the two occurrences of *play* share some semantic features, but not necessarily the whole set. In our example the form *play* in *I play* carries a semantic element "present", which is lacking in *the play*. The shared features form together a semantic minimum (unless, of course, a subsequent comparison shows that they can be further decomposed).

The proportion $boy : girl = boy(ish) : girl(ish)$ reveals in the same way two semantic elements: one common to the two occurrences of *boy*, the other to those of *girl*. Once having established

these, we also have a right to assume the same meaning for both occurrences of *-ish*, without comparing them to any other formal element in these two environments. The evidence is supplied by the fact that the contrast between two sentences, one containing *boyish* and the other *girlish*, but otherwise identical, is exhausted by the contrast *boy*: *girl*. This is a second possibility of reaching positive results in the case of phonemically identical forms in different positions.

Thus far I have neglected the possibility of building a proportion upon the comparison "wood : forest", but no doubt "wood : forest = woods : forests" is valid. Then we regard it as an example of case (2), phonemically different forms in the same environment. Strictly speaking, their distribution is only semantically the same: it can be shown that differences of the phonemic surroundings are automatic consequences of the selection of *wood* or *woods*, for the semantic difference between *this wood is...* and *these woods are...* does not exceed the one between *wood* and *woods*. Before we can derive the semantic minimums, we must also examine the other possibilities of analysis, if any. The two forms share the phonemic element *wood*, so that they can also be considered as an example of case (3). The ending *-s* is then the part of the environment that distinguishes the two positions. If different forms are symbolized as p, q, r, s, and different environments as E, F, the semantic proportion 'wood : woods = forest : forests' can be alternatively symbolized as $p(E) : q(E) = r(E) : s(E)$, or as $p(E) : p(F) = q(E) : q(F)$. The latter approach starts from smaller segments of the sound chain. Therefore it has precedence. We learn from it that *wood* with no ending *-s* and in the combination *woods* contains the same semantic minimum. (As in the previous example, they have not all semantic features in common, for *wood* carries also the meaning "singular".) Consequently, the proportion *wood* : *woods* = *forest* : *forests*, according to case (2), partly consists of the truism *wood* : *forest* = *wood* : *forest*. It goes without saying that one cannot, on this ground, regard the semantic feature resulting from *wood* : *forest* as recurrent. We still find no semantic minimums but "wood", "forest", "singular", "plural".

As concerns case (3), a comparison with the identification of phonemic features in different environments suggests itself. Most linguists simply take it for granted that the dentolabiality of *f* and the bilabiality of *m* are functionally identical, after having observed that they occur in different combinations. In Chapter I, I have claimed that such conclusions should be drawn only after a verification. In semantics, however, the functional validity of an objective similarity cannot be verified by the same kind of experiment. Yet the proportions are well-founded, for here we possess, in addition to the two arguments of objective similarity and complementary distribution also available in phonemics, a third indication: the identity on the other plane (in the form). When this is lacking, we get the fourth case (phonemically different forms in different positions). As a rule this case does not yield proportions sufficient for an identification. We might set up equations like *water* : *swim* = *air* : *fly*, but the lack of formal support makes these constructions a purely personal affair. The question arises then how much formal support is required.

The general formula for case (4) is $p(E) : q(F) = r(E) : s(F)$. If this semantic equation is supported by a recurrence of formal similarity (i.e. if the forms *p* and *q* are similar to each other, and likewise the forms *r* and *s*), we accept it as valid. This is the case in English *sing* : *song* = *see* : *scene*.⁹

Furthermore the same formula $p(E) : q(F) = r(E) : s(F)$ allows one to assume a common semantic minimum for *p* and *q* if we have established on the basis of one of the preceding criteria that *r* and *s* share a semantic minimum. In this case no formal similarity of *p* and *q* is required.

Finally, the relationship *wood* : *woods* can be approached more directly, without an appeal to recurrent semantic features. The two forms are partly alike, they may be symbolized as *p* and *p + q*. If we can demonstrate a semantic relationship $p : (p + q) = \text{genus} : \text{species}$, this will be a firm basis for a semantic identification

⁹ D. D. Bolinger, "Rime, Assonance and Morpheme Analysis", *Word*, VI (1950), pp. 117-136 (p. 119).

of the two p's. For this purpose we examine whether a sentence containing p can be applied to all situations in reality which are covered by an otherwise identical sentence with p + q, but not vice versa. If the answer is positive, the required relationship is established. This is not the case in the example *wood* : *woods*. (I have started from this example because I wish to emphasize that the relationship genus versus species is not to be confused with the one called by Jakobson and his associates unmarked versus marked.) A good instance is the famous *cranberry*.

Thus we employ, in addition to the criterion of recurrence, the criterion of commutability with zero. It can be extended, so that it will include also commutability with a previously recognised semantic minimum.

3. For those linguists who do not assume any submorphemic elements of meaning, the question "linguistically identical or different meaning?" is tantamount to the question "one or two morphemes?" Therefore the subject is generally treated as pertaining to morphology, for example by Nida. Not wishing to attach too much significance to semantic observations, Nida rests his conclusions in this matter on facts of distribution. His principle "Homophonous forms with related meanings constitute a single morpheme if the meaning classes are paralleled by distributional differences, but they constitute multiple morphemes if the meaning classes are not paralleled by distributional differences"¹⁰ is not quite satisfactory, for in order to be a workable criterion, the meanings should not only be related, but also sufficiently different. Otherwise the occurrences of *pencil* designating a red and a black pencil would be two different morphemes. Evidently, Nida considers this semantic difference too small to allow for such an interpretation. It might be argued that there is even here some distributional difference: *pencil* designating a black pencil is impossible in the combination *red pencil*, but we learn from Nida's examples that such distributional differences are regarded as too slight: they are equally present

¹⁰ E. A. Nida, *Morphology: The Descriptive Analysis of Words* (Ann Arbor, ² 1949), p. 56.

in the case of *horn* standing for an animal's horn and an instrument for producing sound, where Nida nevertheless accepts identical distribution. In my opinion, selecting from the various meanings those which are "related, but sufficiently different" is too subtle an operation to inspire confidence. Therefore I think it better to work with proportions, which in the end pretend to be nothing but an extension and feasible definition of Nida's "common semantic distinctiveness".¹¹

¹¹ *Ib.*, p. 7.

SYNTAGMATIC DELIMITATION OF SEMANTIC UNITS

1. We have seen in Chapter II that a morpheme cannot be described directly in terms of distinctive sound features, because its distinctness depends also on the question which segment of its phonetic substance is marked by each feature. So we are compelled to take into consideration a unit intermediate between the distinctive feature and the morpheme: the phoneme. A phoneme is usually defined as a bundle of simultaneous distinctive features of sound (practically, it does not matter whether we also comprise the sound itself into the definition, so that a phoneme will be rather a segment of sound marked by a number of distinctive features).

After this excursion, we more clearly see the question to be answered in the present chapter: Is there any need for positing a unit intermediate between the ultimate semantic constituents and the morpheme? Such a unit is indeed indispensable. The following example will show this.

The morpheme *hammer* contains one semantic minimum. Is this semantic minimum the same in the verb *hammer* and in the substantive *hammer*? It would be tempting to require that the positional variants of a semantic minimum be kept together by a permanent set of features. But this would be too high a demand upon the organization of the semantic pattern. If we find it difficult to establish the exact number of contrasts which define the meaning of an element in a single context, then how can we attempt to compare the totality of them in different contexts? Moreover, there is always the formal sameness, which releases us from the obligation of finding all the evidence in the meaning. Therefore do we not only decide that proportions like *to hammer : to chisel = a hammer : a chisel* are valid, but we go one step farther and assume that the semantic minimum of *hammer* is the same in both environments.

This cannot imply that the *whole* meaning of *hammer* in both cases is identical. The ultimate semantic constituents reflect qualities observed in reality, such as that which distinguishes a hammer from a chisel (and from other things) and hammering from chiseling, and so forth. The whole meaning of a word (or morpheme), however, points to a portion of reality. It would be an inadmissible short-cut to say that a meaning is a portion of reality characterized by a number of distinctive features, but we can certainly maintain that it is an additive entity, as compared with the semantic minimums, which are qualities.

If we compare the realities corresponding to the sentences *he hammers the iron* and *he buys a hammer*, we notice in both of them in some way hammer-qualities, but the things characterized by these qualities are entirely different. There may even be no hammer at all (that is, when we interpret the verb as "to beat *as* with a hammer"). The reason is that a semantic minimum can be differently spread over the substance. When the word *hammer* is a substantive, it points to one thing, a hammer. But when it is a verb, it implies two things, one beating, the other beaten, and neither of these is as a matter of fact a hammer. These two things carry together the hammer-quality. We may say that *to hammer*, as a transitive verb, expresses a relationship, but what we really mean then is that the word points to two things in a given relationship to each other. When we further say that *he* and *the iron* are the subject and the direct object of the finite verb *hammers*, this way of speaking does not make any sense if we understand it in such a way that the things expressed by these parts of the sentence stand in a certain relationship to *hammers*, which in itself is already the expression of a relationship: they rather serve to specify the things already contained in the meaning of *hammers*, they indicate what the terms *x* and *y* of the relationship expressed by the verb stand for. In other words, the grammatical phrase "*The iron* is the direct object of *hammers*" means: the thing pointed to by *the iron* is the same thing as the one pointed to by the term *y* contained in *hammers*. Consequently, the distinction of two things in the reality corresponding to *hammers* (and the distinction of two semantic

units in the meaning "hammer" in this construction) is necessary. I have proposed for these units the term "valences".¹ The semantic minimum "hammer", which occurs undivided in the substantive, is split up into two valences in the verb. The meaning "hammer" may be called "monovalent" in the substantive, and "bivalent" in the verb: in the former case it is to be applied to one thing in reality, in the latter case to two.

To sum up: the meaning of the morpheme *hammer-* in *he hammers* evidently consists of two distinct units ("valences"), although the same semantic feature ("semantic minimum") extends over both of them, in the same way as the form *-ts* in *cats* evidently consists of two distinct units ("phonemes"), although the same distinctive feature (voicelessness) covers both of them. The discursive nature of language makes it often necessary to point repeatedly to the same thing in reality, so that one thing is referred to by a number of semantic entities; in the example *he hammers* this applies to the entities "hammering" (one of valences of *hammer-*) and "he".

Thus an utterance consists of two strings of additive entities, linked to each other by the rules of the code: a chain of phonemes and a sequence of semantic entities. The term "semantic entities" comprehends both the valences and the monovalent semantic minimums (or the monovalent occurrences of semantic minimums). A semantic entity being rather often characterized by only one semantic minimum, the difference between these two types of constituents (thing versus quality) is less outspoken than in the case of phoneme and distinctive feature.

2. When speaking of "same position" with a view to establishing semantic entities, I meant all the time the arrangement of the semantic sequence. It is not the place here to go into details, but one point deserves being mentioned. When we break down a meaning on the basis of a comparison with a form in the same position, the result should be a qualification, not the addition of a

¹ In the article "On the Semantic Structure of the Russian Sentence", *Lingua*, IV (1954), pp. 207-222.

valence. So a stallion is at the same time a male being and a horse: these two semantic entities qualify each other. But a proportion "fish : water = bird : air" is unacceptable, for the resulting decomposition would interpret a fish as something standing in a certain relationship to water, so that it would be a bivalent entity. It is precisely the fact that they occur in different positions which enables the linguist to recognize the same meaning in the substantive and in the verb *hammer*. What is identical is, in terms of Hjelmslev's quadripartition, the form of the meaning (or a part of it), not the substance. This semantic entity ("sememe") has different contextually conditioned realizations ("allosemes"). So "hammer" is realized alternatively as "hammer-like object" (the noun), "hammer-like acting" and "hammer-like being acted upon" (the two valences of the verb). The action associated with a hammer happens to be beating, and this belongs also to the realization. Theoretically, "hammering" could as well be "molding into the shape of a hammer" or "attempting to catch hammers". A linguistic description should pay attention to the realization of a meaning in a given context: it is no less important than, for example, the aspirated pronunciation of the *p* in certain positions in English.

When we study differences of meaning in a given context, we can, of course, only compare realizations. If we pin ourselves down to the interpretation of "hammering" as "beating", a proportion like $(a) \text{ hammer} : (a) \text{ fish} = (I) \text{ hammer} : (I) \text{ fish}$ will have to be rejected. But once we realize that "beating" is completely absent from the occurrence as a noun, the proportion proves to be valid, and we decide that $(a) \text{ fish}$ and $(I) \text{ fish}$ share a semantic entity. (To be sure, the same conclusion may be drawn on the basis of a narrower description of the meaning, for there is a recurrence $(a) \text{ fish} : (a) \text{ crab} = (I) \text{ fish} : (I) \text{ crab}$.) Thus we ascertain a common element in the linguistic meanings of *I bought fish* and *She fishes for compliments*, although the things meant have hardly anything in common.

The conclusions of this chapter are the following: since bivalence is a matter of semantic arrangement, (1) we can ascertain it only when the semantic whole unmistakably indicates it, and (2) we

can analyze a semantic minimum alternatively as monovalent and bivalent when this corresponds to a difference of distribution.

Breaking down a semantic entity by comparing it to another entity in the same position is possible only when the two parts qualify each other. I employ this term here in a narrowly defined sense. It applies to the elements "horse" and "male" contained in "stallion": if we select from all thinkable things the horses and the males by two independent procedures, the things belonging to both selected groups are the stallions. The term does not apply to the elements "very" and "large", which we might abstract from "huge". Therefore I regard the latter analysis as not justified (this relationship might be called "specification").

THE MORPHEME

1. The morpheme is not generally recognized as a linguistic unit. One of the reasons for the guarded attitude towards it is no doubt to be looked for in the difficulty of deciding in every single case where a morphemic borderline must be drawn. This applies to the vertical cuts in the chain of the utterance, i.e. to the syntagmatic isolation of the morphemes, as well as to their paradigmatic delimitation. The most extensive and thorough study in this field is Nida's well-known textbook.¹ It is a very rich book containing a carefully worked-out procedure of morphemic analysis with a number of neatly formulated criteria. Nevertheless, it still leaves certain problems unsolved, as is rightly remarked by Martinet (see below, p. 114).

The morpheme is not one of the units which everybody is intuitively inclined to distinguish, such as the word or the sentence. Therefore the difficulties connected with its delimitation are harder to accept than in the case of the latter units, where they are not easier to overcome.

But there is a better argument in favor of the morpheme than intuition: theoretical cogency. A linguist who holds the view that a language is a code, is forced to discover minimum units in which the semiotic relationships are established. These units are two-sided, they form the bridge between sound and meaning. Theoretically, the necessity of reckoning with morphemes cannot be denied.

The difficulties, moreover, are not insurmountable. In current theories only phonemic analysis precedes the search for the morphemes. I consider this a serious shortcoming, for in this way the

¹ Nida, *Morphology*...; unless indicated otherwise, the quotations from Nida in the following pages refer to this book. See also E. A. Nida, "The Identification of Morphemes", *Language*, XXIV (1948), pp. 414-441.

tackling of the morpheme is prepared only from one side. On a lower level of analysis there is a linguistically satisfactory possibility of dealing with meaning, too. I hope to have shown this in the foregoing chapters. When we approach the morpheme with the knowledge derived from this semantic analysis, we are much better equipped than when we start with a vague, amorphous mass on the side of the meaning.

Things are considerably complicated by the existence in linguistic literature of three different types of morphemes. The first, in a certain sense the ideal type, is the one where a portion of the sound chain corresponds to a portion of the content, as in *boy-ish*, *work-ed*, *cat-s*, each consisting of two morphemes.

The second type of morphemes, as we find it in current theories, consists of an element of meaning expressed by a formal element, as in the first type, but this formal element is not a segment of the sound chain. It may be a "significant absence of a formal feature" (Nida, p. 46). Nida uses here the term "zero". An example is "plural" in *sheep*. The case of *feet* (: *foot*) belong also to this type. According to Nida, "we may say that *feet* actually consists of three morphemes: (1) the stem, (2) the replacement of /u/ by /iy/, and (3) the zero suffix" (p. 54). If this is true, neither (2) nor (3) represent a section of the sound chain. Morphemes like (2) are called by Nida "replacives" (p. 55).

The third type of morphemes is exactly the reverse of the "replacives": a distinct segment of the linear form is involved, but, as to the meaning, no clearly delimited element can be indicated which corresponds to this section of sound. In English many prefixes are of this type, e.g. *be-*, which is connected with a meaning in verbs like *be-smear*, *be-wail*, *be-dazzle*, *be-little*, but which completely changes the meaning of the simplex in *be-hold*, *be-come*, as compared with *hold*, *come*. Roughly speaking, we might describe the common semantic element of *come* and *become* as "to reach a point", and the change of meaning caused by the addition of *be-* as a replacement of a semantic element "literal", present in *come*, by "figurative". In other verbs, even such a replacement cannot be shown: if we attach the prefix *be-* to the verb *gin* (*gan*, *gun*), the

only result is that we get a normal word instead of a stylistically colored one. If we regard the stylistic feature as an element of meaning, the addition of the prefix conveys the subtraction of a semantic element. In this way the word *begin* is marked by a "significant absence of a semantic feature", and resembles the case of *sheep*: in *begin* there is a separable part of the sound chain which lacks content, in *sheep* the semantic element 'plural' does not find an overt expression.

It is this varying aspect of the morpheme, I believe, which induces some scholars to reject Bloomfield's acceptance of the term. The Bloomfieldians tend to generalize the first type and to regard the morphemes in the first place as parts of words (or as whole words, as the case may be). For them, the second type forms a special case. Other linguists, not satisfied by this approach, take the opposite stand – this view is, as far as I know, most clearly advocated by De Groot – and generalize the second type. Thus, they define the morpheme as the differential feature in an opposition between two words.

When one wishes to settle the controversy, the question to be answered runs, briefly formulated: is a morpheme (1) a quality of a word or rather (2) an additive entity?

Both views have their inconvenience. According to (1), the *-s* of *cats* is a *feature* of the word by means of which it is distinguished from *cat*. According to (2), the replacement of the vowel in *foot* (so that we get *feet*) is something which is added to *foot*. At first sight, view (1) seems to strain the facts less than (2), for the latter requires a notation of the morpheme which will inevitably be rather cumbersome, like the one proposed by Nida (p. 55): /iy←u/. Moreover, view (2) makes it necessary to recognize one form as basic (in the given instance: *foot*) and the other as derived (*feet*), whereas (1) allows for the possibility that both *cat* and *cats* carry a differential feature. This necessity is implied in the term "replacive", and obviously it would make no sense to interpret also *foot* as containing a morpheme /u←iy/. To be sure, a relationship basic versus derived may be assumed for *foot* versus *feet* in view of the semantic similarity of the difference between *cat* and *cats* on the

one hand, and *foot* and *feet* on the other; this assumption presupposes the interpretation of *-s* in *cats* as an additional element. A reference to the meaning ("plural" in itself being derived from, or based upon "singular") is not customary in this connection. Yet I think it preferable to leave out considerations of this kind, because they unduly extend the level of morphemic analysis, and, moreover, raise new questions, as for instance: once assuming the existence of zero suffixes, why not accept a zero ending for the singular *cat*? This would make the direction of the derivation ambiguous again.

This whole argumentation, however, is of a practical nature. It is inspired by the desire to supply a set of rules, as unequivocal as possible, by means of which a language can be described in the easiest way, so that the result will be maximally informative. We have scarcely touched the theoretical adequacy of the reasoning. Yet theoretical considerations should prevail. In this regard view (1) is definitely weaker. When the morpheme is taken as a differential feature of a word, morphemes can be detected only there where two words are in direct opposition to each other. Such an opposition is present in the case *cat* : *cats*, and so we arrive at morphemes with the meaning "singular" and "plural". But the meaning "cat" will inevitably remain outside the inventory of morphemes. In other words: view (1) lets the meaning "plural" appear in the language already at the level of the morpheme, the meaning "cat" only at the level of the word. This leads back to the old dichotomy of "morphème" and "sémantème" (Vendryes), and thus cuts artificially a linguistic level into two distinct parts, viz. the level where sound and meaning fuse into a sign. Such a procedure would be contrary to the notion of language as a code. I do not deny, of course, that different subtypes of morphemes can be ascertained, and that the difference between two of them may resemble the distinction "morphème : sémantème", but a linguist has to pay attention in the first place to the unit which embraces these subtypes, and since the term "morpheme" is now commonly accepted in the sense attached to it by Bloomfield, it is not advisable to follow the terminology of Vendryes.

The theoretical reason why the morpheme is important for a

linguist is that it is the smallest unit where sound is associated with meaning. At the level of the morpheme the semiotic function of language begins to operate. Linguistic signs consist of a chain of phonemes (or one phoneme) corresponding to an assemblage of semantic minimums in a certain arrangement (or one semantic minimum), but the larger signs (words, sentences, utterances) have this two-sided character because they are composed of units which already have it themselves, minimal signs. The minimal sign is the morpheme.

Anyone who wishes to describe a language must investigate which segments of the sound chain are endowed with meaning. The sound chain is a string of phonemes which are grouped together into formal halves of signs. The semantic halves of the same signs are arranged into more complex constructions. (The arrangements fall outside the scope of this study.)

This exposition contributes a new argument in favor of the view that morphemes are additive entities: they are, through their formal halves, constituents of a *linear* sound chain. In this sense, neither zero morphemes nor "replacives" can be interpreted as constituents of the utterance.

I do not very well see why a linguist should not be allowed to reproduce the facts as they offer themselves to him, why he should be obliged to represent at any price as equal what in reality is different, why he should mold a notion in order to adapt it to different phenomena. Given the nature of language, the expression of the meaning "plural" is basically different in *feet*, *sheep* on one hand, and *cats*, *oxen* on the other. If one wishes to employ the term "morpheme" in the sense of "minimal sign", it can only be applied to the latter cases. If one prefers to regard it as a feature of a word, it will be no longer a minimal sign, and the linguist will have to coin a new term for this fundamental notion.

As to *feet*, there are only two reasonable analyses: it may either be cut into three morphemes (or – what amounts to the same thing – into two, the one inserted into the other) /f-iy-t/, or be interpreted as one morpheme. The former analysis yields a morpheme /iy/ = "plural", while in the latter analysis the morpheme is coextensive

with the word: /fiyt/ = "foot + plural". I agree that a morpheme *feet* is related, both by a semantic and formal resemblance, to *foot*, but I doubt whether this is a solid basis for isolating morphemes, for there are numerous instances of a similar relatedness where nobody would think of drawing a morpheme border. In this respect I need only refer to an interesting article by Bolinger, who presents something like a *reductio ad absurdum* of current methods in morphemic analysis.² Nida, too, quoting Read, signals cases like *fake* and *fakir*, *bust* and *robust* (p. 57, n. 51). I do not consider this relatedness a property pertaining to the code; it may be of great help for memorizing words belonging only to the passive vocabulary and perhaps play other roles as well, but nevertheless such morphemes are linguistically independent. To be sure, the two morphemes *foot* and *feet* share a semantic minimum ("foot"), but this pertains to the semantic analysis which preceded the ascertainment of the morphemes: it certainly finds its place in a description, but not in the treatment of the morpheme level. It rests on the fact that the semantic relationship of *foot* and *feet* is recurrent in the language, but this holds also good for *see* and *scene* (at least according to Bolinger [*o.c.*, p. 119], who sets up the equation *sing* : *song* = *see* : *scene*) and is consequently not an argument for giving the semantic elements a morphemic status and laying the form on the Procrustian bed of the thus modeled meaning. It may further be objected that the description of languages with prevailing "inner flexion" (such as Arabic) would in this way deviate too much from the normal pattern found in Indo-European languages, but I am convinced that if the languages themselves have striking differences, these must appear in the descriptions, too.

Also, the question whether the past tense *put* is linguistically different from the homonymous present concerns merely the identification of semantic minimums, for which we have proposed the criterion of discreteness, as treated in Chapter III. Since this criterion points to their distinctness, we ought to list two related homonymous morphemes *put*. The same applies to *sheep* = "number-irrespective sheep + singular" and *sheep* = "idem + plural".

² Bolinger, "Rime, Assonance. . . .".

If we separate the prefix of *be-long* as a morpheme, it is impossible to avoid a terminological difficulty, for *be-* is at any rate not a minimal sign, it lacks the semantic half. This does not mean that it is not reflected on the semantic plane. It certainly has some semantic function, as will be shown presently, and since it is formally detachable, it must be separated. If we maintain the term "morpheme" also for this kind of unit – and it is advisable to do so, for it is in accordance with current usage and does not present any practical difficulties – we will sometimes have to specify whether we mean full morphemes, consisting of a segment of the sound chain plus the corresponding semantic minimum(s), or formal morphemes (or half morphemes), consisting of a form without a directly corresponding meaning.

2. Any discussion of the morpheme should – and most linguists indeed do, like Nida (pp. 6-7) – start from Bloomfield's excellent definition: "A linguistic form which bears no partial phonetic-semantic resemblance to any other form is a *simple* form or *morpheme*." ³

"Resemblance" is to be taken in the sense of "relevant resemblance", for only thus has it an objective value. Relevant resemblance means (when we develop the thoughts of the previous chapters of this study): identity of phonemes and semantic entities. This implies that the form analyzed into phonemes and the meaning analyzed into semantic entities should serve as the point of departure for morphemic analysis.

The first thing we learn from Bloomfield's definition is that in the analysis of a language on the morphemic level, the study of the paradigmatic axis has to precede the study of the syntagmatic axis. In other words, the recognition of the resemblances furnishes the directives for the decomposition of the chain into morphemes. We first trace, so to speak, the relevant morphemic features, and only their discovery enables us to isolate the morphemes. In this respect the strategy is the same in morphology as in phonology and in semantic analysis.

³ Bloomfield, *Language*, p. 161.

A relevant morphemic feature presents itself to the investigator as a correspondence between one or more phonemes on the one hand and one or more semantic minimums on the other. This is our phrasing of a second point to be retained from Bloomfield's definition: it speaks of a phonetic-semantic resemblance, not of a phonetic resemblance plus a semantic resemblance. It must be possible to show which part of the form corresponds to which part of the meaning; some identical phonemes in combination with a semantic similarity in general do not meet this requirement.

In this manner one eliminates the problems connected with phonetic symbolism, which are left open by Nida's method of analysis. Nida does not cut the word *flare* into two morphemes, which induces Martinet to ask in his review of the book: "But what about the proportion *flare, flimmer* : *glare, glimmer*? Would it not seem to point to an *f*- and a *g*-morpheme (or should we rather say *fl*- and *gl*-)?"⁴ It depends on whether the proportion set up by Martinet is also semantically valid. This is obviously not the case, as we learn from the procedure of Chapter II: a foreigner supplied with this proportion and with information about the meaning of three of the terms, will still be unable to infer the meaning of the fourth.

At this point it seems proper to quote a passage from Voegelin: "To find the meaning of morpheme *A* and morpheme *B*, we may be forced to deal with the sequences *A + X* and *B + X*, and we often wonder what the morpheme *X* contributes to the total meanings of these sequences – the meanings from which we expect to abstract the separate meanings of *A* and *B*."⁵ By starting the search of the morphemes *after* having subjected the semantic constitution of the forms to a verifiable investigation, we change this doubt into a certainty: we find in the stem of *flare* only one semantic minimum. Consequently, we observe only one form-meaning correspondence in this word. We understand then the word "partial" in Bloom-

⁴ A. Martinet, Review of Nida, *Morphology*...—*Word*, VI (1950), pp. 84-87.

⁵ C. F. Voegelin, "Distinctive Features and Meaning Equivalence", *Language*, XXIV (1948), pp. 132-135, footnote 3.

field's definition as "in a part of its semantic minimums and in a part of its phonemes". In these parts the identity should be complete, not approximate. "Partial" had to be taken in its quantitative sense ("pertaining to a part"), not as a qualitative restriction ("nontotal").

The form-meaning correspondences are often very easily established. Many of them are immediately given, e.g. the form of a whole sentence such as *I saw a cat* corresponds to its meaning. For our purpose it is necessary to analyze such correspondences until we get the minimal ones, which we need for the analysis of the utterances into morphemes. The correspondences which are part of a larger whole are recognized in the shape of *recurrent* formal and semantic segments (the exact procedure will be given below). For example, when we compare *cat* and *cats*, we find a same semantic minimum "cat" and a same sequence of phonemes; a comparison of *cats* and *ducks* yields the semantic minimum "plural" and one common phoneme; thus we have two correspondences in the word *cats*, and all its relevant semantic and phonetic elements participate in one of them. In this example the correspondences are by themselves sufficient for analyzing the word into morphemes: *cat-s*.

Not in all cases, however, are the correspondences conclusive for the analysis. The word *houses*, compared with *house*, shows a same semantic element "house" and a same phonemic part which may be symbolized as /hawS/ (where the capital /S/ stands for a voice-irrelevant sibilant). A comparison of *houses* and *cats* furnishes a correspondence: "plural" = /S/. These correspondences do not lead directly to isolating morphemes, for a borderline pertaining to morphology cannot be supposed to cut through the units of a lower level of analysis, a morphemic dissociation of a phoneme /z/ into voicing + /S/ cannot be admitted. Moreover, we are left in this way with a part of the form which does not belong to either of the correspondences: voicing + /i/ + voicing.

The difficulty is usually solved by making a distinction between *morphemes* and *morphs*, the latter being the actualizations of the former. (I shall not follow this terminological practice and use

throughout this study only the term "morpheme" in both senses, for mostly the context makes clear what is meant. Only when the distinction itself is discussed, I shall keep the two terms apart, but then with quotation-marks.) The "morphemes" are invariants which admit different realizations. So the established correspondences are fully conclusive also in this type: the word *houses* consists of two "morphemes", and it is senseless to require that the realizations of these "morphemes" be constant as to their phonemic make-up. We have found two English "morphemes" meaning "house" and "plural", which we might symbolize by means of hieroglyphs or ciphers, but which cannot be described in terms of phonemes. They are formally characterized by the presence of at least one phoneme or even only of some distinctive features.

Having thus established "morphemes", we do not yet know the "morphs": in the given instance, must we analyze /hawz-iz/ or /hawzi-z/? Here a fundamental question arises. Are the borderlines between the "morphs" an indispensable part of a language description? It may seem doubtful when we consider only examples like this one. But other cases will show that we have forgotten one important aspect of the problem. A comparison of *blow*, *blew*, *throw* and *threw* unambiguously reveals the correspondence /ow/ = "present", but this does not lead necessarily to the separation of a morpheme /ow/, for it cannot be denied that the phonemic opposition /ow : uw/ may as well be regarded as distinguishing these words as a whole, exactly like *row* and *rue* are distinguished by these features (no matter in what meaning we take these words). Only the fact that *blow* and *blew* present semantically a minimal opposition might induce us to ascribe a meaning to *bl-* and *-ow* separately, but a language can even here very well dispense with such partitions, as clearly appears from cases such as *go*, *went*, and this makes the analysis into *bl-* and *-ow* doubtful again. The procedure which would jump from the correspondences immediately to the "morphemes" and skip the "morphs", neglects this problem and is for that reason insufficient. We need a special procedure for interpreting the correspondences in terms of morphemes, for not all correspondences are, so to speak, relevant morphemic features.

If the morpheme is defined as a union of two corresponding segments, of the phonetic and the semantic sequence respectively, the isolation of which is structurally necessary, we arrive at a comparatively simple procedure, consisting of a number of consecutive steps. In the formulae used here the italics *a*, *b*, etc. indicate sequences of phonemes (or single phonemes), while "a", "b", etc. stand for semantic minimums or groups of them.

If we find on one hand a form *a* corresponding to a meaning "a" and on the other *a + b* corresponding to "a + b", a morpheme border is to be drawn between *a* and *b*, provided the form *b* is detachable from *a* by a single vertical borderline (i.e. if the one is not inserted into the other or superimposed). So we get the analyses *duck-ling*, *cran-berry*.

The restriction of detachability is dictated by the conviction that the formal side of a morpheme, as a part of a linguistic utterance, shares the linear character of the latter; it can hardly be assumed that the morpheme differs in this respect from other formal units (phonemes, forms of words, forms of sentences), which are also arranged in strings. We do not exclude in this way the possibility of discontinuous morphemes, but Nida's replacives (see above p. 108) are too exceptional to be acceptable. An inserted morpheme can be ascertained when it is in other combinations detachable (for an example see below p. 119).

This analysis is the first step of the procedure. It cannot be repeated off-hand after the subsequent parts have been completed. The importance of the order of the consecutive steps may be illustrated by an example. We shall discover a morpheme ("morph") /*mei*/ with the meaning "tense-irrespective making" in *made*, but we cannot regard this /*mei*/ as a possible *a* in a symbolization of this type, so that *make* would present *a + b*. Otherwise we would have to assume a morpheme /*k*/ with the meaning "present". The mere correspondence /*k*/ = "present" cannot be denied, for /*k*/ of *I make* no doubt indicates that not the past tense of the verb is meant, but this state of affairs is evidently not sufficient for establishing a morpheme.

For discovering in this way recurrences of correspondences, a

complete phonemic identity of the two forms *a* is not required in the formulae $a = "a"$, $a + b = "a + b"$. The complete identity of the semantic minimums represented by "*a*" is a sufficient support for positing the recurrence. So a pair of words like *goose* and *gosling* perfectly fits the formula, although the form for which the correspondence is ascertained is only /g/ + vowel + sibilant. *Goose* and *gos-* are realizations of the same morpheme.

I want to stress, perhaps superfluously, that the analysis is based upon an identification. Immediate data are the two correspondences *goose* = "*goose*" and *gosling* = "*goose* + young" (the analysis of the meaning "*gosling*" into two semantic minimums is justified by the criteria of Chapter II). The recurrent semantic element "*goose*" asks for a comparison of the phonemic material: a part of the phonemes turns out to be common to both expressions, and thus nothing prevents the identification of the correspondences, and, as a consequence, the assigning of morphemic status to the elements *gos-* and *-ling*.

Various categories of morphemes can be distinguished. First of all, there is the difference between what we may call, following the terminology of the Bloomfieldians, "free forms" and "bound forms". *Goose* is a free form. The *a* in the part of the above formula $a = "a"$ should always be a free form. The "morpheme" meaning "*goose*" has a free form /guws/ and a bound form /gaz/ as formal "allomorphs". Other "morphemes", such as *-ling*, have only bound "allomorphs".

More important for the procedure is the classification of the morphemes according to the way they are ascertained by the investigator. The analysis of *gosling* has been performed on the basis of a recurrence of the correspondence contained in the first part: it is also found in *goose*. I propose to speak of a "recurrent morpheme". The second part, *-ling*, is nothing but what is left over after the subtraction of *gos-*: it may be termed a "rest morpheme". It should be borne in mind, however, that what appears as a remainder at the first step of the analysis, may turn out to be a recurrent morpheme in the subsequent procedure, as is the case with *-ling*, but it is characteristic of certain morphemes that they

do not reach the status of recurrent morphemes throughout the whole procedure, e.g. *cran-* from *cranberry*.

The procedure is continued as follows: if $a + b = "a + b"$ and $a + c = "a + c"$, we analyze accordingly, e.g. Latin *urb-ēs*, *urb-ibus*. When b or c is inserted into a , it can be severed only if it is elsewhere in the language attested as indubitably detachable. This is obviously not the case in *goose*, *geese*, and we have here in each form only one morpheme. Avar *dova* "here" (locating a male), *doba* "here" (with reference to an inanimate thing) must be split into three parts *do-v-a*, *do-b-a*, for we also find *v-ugo* "he is", *b-ugo* "it is". Whether we regard *do...a* as two morphemes or as one discontinuous morpheme, is just a question of taste.

Before attaching any significance to a relation $a + b = "a + b"$ as to its morphemic make-up, we must be certain that b is really an addition to a , that it does not merely help to characterize the form $a + b$ as a whole. Otherwise we might be tempted to analyze *bl-ow* = "blowing + present", as compared with *bl-ew* = "blowing + past". We notice, however, that in other contexts the meaning "present" has unquestionably a zero expression (e.g. *I work*), so that there is also a possibility of comparing the correspondences: (zero) = "present" and *blow* = "blowing + present", where the semantic minimum "present" is recurrent. It appears that the correspondence (zero) = "present" is perfectly admissible in the case of *blow*, too, and the other possible correspondence *-ow* = "present" can for that reason not be projected on the morphemic level: "present" is a part of the meaning of the form as a whole in certain contexts, not of one of its segments. Perhaps it would have been better to choose the example *see* : *saw* instead of *blow* : *blew*, for then we would have avoided the possibility of recognizing the same final consonant in the present and past forms. The verb *blow* is preferred because its conjugation is not unique, it has its like in *throw*. Thus the example shows that mere recurrence is not sufficient for positing a morpheme border. Both correspondences (*-ow* = "present" and [zero] = "present") are equally recurrent, and only one of them can be used for the morphemic analysis.

The morphemic cut in *bl-ow* is not compulsory, and therefore it has to be rejected.

The morphemes *see* and *saw* must then be listed as two *related* morphemes (see above p. 112). They bear a phonetic and a semantic resemblance, but these cannot be shown to be coextensive. There is one morpheme *saw* containing two semantic minimums "seeing + past". In my opinion, it is merely a trick to assume for *saw* a second formal morpheme *zero*; it may be convenient for some purpose, but it is by no means necessary.

The form *make* has a better chance of being broken down. To be sure, the comparison *make* : *made* is as insufficient as *see* : *saw*, but the form *made* consists of two morphemes, and this makes the comparison much more weighty. Yet the analysis is indefensible, for *ma-* is a rest morpheme and as such incapable of procreating a new rest morpheme /k/.

The comparison of Latin *novus* "new" (nom. sing. masc.) and *novī* "new" (gen. id.) is fully adequate from a morphemic standpoint. It cannot be denied that the semantic element "nom. sing. masc." is often expressed in Latin by a zero ending, as in *miser* "unhappy", but a further comparison of the genitive *novī* and the dative *novō* offers a recurrent morpheme *nov-*, so that the analysis *nov-us* is justified.

A third criterion in the procedure may be symbolized as follows: if $a = "a + b"$ and $a + c = "a + c"$, we list two morphemes $a = "a"$ and $c = "c"$. An example is: *pay* = "paying + present", *pai-d* = "paying + past". In a single example the segment *-d* is a mere remainder, but parallel relationships, such as *owe* : *owed*, attest that it is a recurrent morpheme. Therefore we separate *-d* by a morpheme border also in the word *made*. In *saw*, on the other hand, the element *-aw* cannot be isolated, for the meaning "past" is in English not always expressed by a separate morpheme (*put*, *took*), and we look in vain for an indication that either *s-* or *-aw* is a recurrent morpheme. Even if a final segment recurs, as *-ew* of *blew* recurs in *threw*, we have no right to interpret *blow* and *bl-* as allomorphs of one "morpheme", and *-ew* as an ending morpheme, for in the formula $a = "a + b"$, $a + c = "a + c"$ complete

phonemic identity of the two occurrences of *a* is required in all cases where "c" occurs elsewhere in the language without a separate corresponding formal morpheme.

With respect to the demonstrative force of the formula $a = "a + b"$, $a + c = "a + c"$, an important restriction must be made. It may happen that the first and the second *a* on closer inspection turn out to be homonyms. This is indeed the case in *[he] put* = "putting + past", *[he] puts* = "putting + present", for *I put* occurs in the same contexts with two different meanings. It is true that the *-s* of *puts* indicates which of the two morphemes is pertinent, and thus indirectly has a semantic function, but the meaning "present" nevertheless remains an inherent part of the morpheme *put*.

3. The criteria treated thus far serve only to ascertain those morphemes which are minimal signs, i.e. which are represented by detachable elements both in the form and in the meaning. But, as said above, there exist also morphemes participating only in one plane: the formal morphemes. These require a few additional criteria.

The word *deceive* exemplifies this phenomenon. Nida divides it into two morphemes, but there is obviously only one semantic minimum. Nonetheless, the analysis appears to be sound. Sometimes the element *de-* certainly conveys a meaning, as in *decompose*, *decomposition*. In *deceive*, *deception* we observe a phonemically identical initial segment, but no meaning can be ascribed to it, for *-ceive* has no separate meaning and, consequently, *de-* cannot be said to modify the meaning of *-ceive* in any way. However, on the basis of the phonemic identity and the occurrence in analogous positions (i.e. in the same environments with the exception of one contiguous element), the two segments must be identified, and we have to list a formal morpheme (an "empty morph") *de-* in *deceive*.

Another example: Dutch words with the diminutive suffix *-tje* (such as *heuveltje* "hillock"; compare *heuvel* "hill") share their internal construction and external distribution with a word like *dubbeltje* "dime", which does not occur without the suffix. Therefore this word has to be dissociated into two morphemes.

The demand of analogous distribution precludes the disintegration of words like *hammer* and the identification of *-er* in *dancer* and *hammer*. If we read Nida carefully (*Morphology*, pp. 58-60), we learn that he thinks one argument decisive for refusing morphemic status to the constituent parts of *hammer*: "This *-er* is not semantically relatable to the agentive *-er* in *dancer*, *player*, *runner* and *walker*." This leads the reader to supposing that Nida regards *con-* in *conceive* as semantically relatable to *con-* in *condense* (or we would have to assume that he does not make a similar requirement in the case of *con-*). This seems a rather weak ground for making a distinction. Therefore I would prefer the following reasoning. In *danc-er*, etc., the first morpheme may have its own syntactic relationship with other elements in the sentence (as in *tapdancer*), whereas *hammer* participates in more complex structures only as a whole. Consequently the two elements *-er* do not have analogous distribution, and *hammer* cannot be broken into two morphemes. This case shows that the morphemic analysis cannot be completed until the arrangement of the semantic minimums has been studied.

Since an "empty morph" of this kind contributes to the formal characterization of a larger unit as a part of the lexicon, the term "lexical index" seems appropriate. So we register for English homonymous morphemes *-ceive*. Which of them is meant is indicated by an accompanying meaningless morpheme *de-*, *re-*. Likewise, there are three homonyms *long*, one of which is recognized by a prefixed *be-*. The prefix has the same function as might have a subscript cipher in a morpheme list, e.g. *long*₁, *long*₂. A lexical index is often added to a morpheme which does not seem to need it, as in *bequeath*, where the procedure nevertheless leads to its isolation. If we follow this procedure, the recurrence of the element *-ceive* does not serve as a criterion for the morphemic dissociation.

There is one more type of lexical indexes. It is established on the basis of a comparison with a full morpheme. The conditions are phonemic identity and same immediate environment. An example is the Dutch prefix *ge-*. It is a mark of the past participle, e.g. *ge-maak-t* "made" (compare the infinitive *mak-en* and the present

tense *maak*). The same phonemic sequence appears in the past participle *ge-bruik-t* "used", but here it is found also in the other forms of the verb, such as the infinitive *ge-bruik-en* and the present tense *ge-bruik*, and even in the substantive *ge-bruik* "use". Although the element *ge-* in the participle cannot be said to distinguish this form from the other forms of the verb, it is difficult not to identify it with the prefix of *ge-maak-t* as a lexical index of the first type, occurring in analogous positions. But once we have isolated *ge-* in the past participle, we should regard it as a separate morpheme in the other forms, too. The sameness of position is restricted here to the one contiguous morpheme *-bruik*.

Not all of the "empty morphs" of a language fall under the category of lexical indexes. There is another type, for which I would propose the term "syntactic indexes". The Georgian suffix *-ma* furnishes an excellent example: it indicates that the noun or pronoun to which it is attached serves as the subject to a verb, mostly a transitive one and nearly always (with one exception) in the aorist. The element *-ma* is, so to speak, the mortar binding the subject to the predicate, but it does not convey any meaning of its own. The same can be said of English /z/ in *goes*: it is always there when "he" is combined with "going" and "present" into a subject-predicate group, and thus it indicates the syntactic relationship. Likewise, the ending *-us* of *nov-us* in Latin *homo novus* informs the listener that *nov-* "belongs" to *homo*; we draw this conclusion from the mere fact that its presence in this construction is obligatory, although it does not contribute an element of meaning.

In the procedure for ascertaining morphemes, syntactic function has the same value as meaning: in all formulae the symbols "a", "b" and "c" may be regarded as representing syntactic functions. Thus Latin *novus* = "new" + syntactic function relating "new" to the meaning of *homo*, *novī* = "new" + syntactic function relating "new" to the meaning of *hominis*, is a solid basis for establishing morphemes. It goes without saying that not all correspondences of a phonemic element and a syntactic function compels us to separate a morpheme: the same restricting rules must be observed as in the case of form-meaning correspondences.

A syntactic index is a segment of the *form* which indicates the *semantic* relationship between two elements. I do not apply the term to a segment of the form which binds two elements always *formally* together (i.e. stands in the sound chain in immediate contact with both of them), although it is in these cases at the same time an attendant symptom of a semantic relationship. Thus in Hungarian *dolgok* "things", *dolgom* "my thing", *dolgod* "thy thing", as compared with *dolog* "thing", I do not consider *-o-* a syntactic index. Other words, such as *fák* "trees", show the right morphemic analysis of *dolgok*. These Hungarian noun forms have been the subject of a controversy between Bergsland and Hall.⁶ Comparing *dolgok* "things" and *dolguk* "their thing", Hall assumes an "auxiliary vowel" or "empty morph" which in these forms appears alternately as *-o-* and *-u-*; "it is quite true", he adds, "that this substitution [of *-u-* for *-o-*] is a major factor, though not necessarily the only one, in helping the hearer or reader to identify the meaning of the suffix that follows". This following suffix is then either *-k* "plural object" or *-k* "plural possessor". Making a comparison with phonemics, we might regard *-o-* and *-u-* as containing a "redundant" morphemic difference, serving only to identify an other element. There is, however, no cogent argument for doing so, and Bergsland proposes to distinguish two separate stems, one of them indicating possession. Thus, I infer, the two *-k*'s can be identified, they both mean "plural". The opposition *dolgo* : *dolgu-* is reflected in the meaning. Since the element *-o-* occurs also in *dolgom* "my thing", etc., it is definitely deprived of any meaning, and consequently not a morpheme. But *-u-* clearly conveys the idea "possession". So *dolgok* is morphemically *dolog* + *k* "plural thing", while *dolguk* consists of three morphemes *dolog* + *u* + *k* "plural possessor's thing". As this discussion shows, I deny the existence of "empty morphs" which are neither syntactic nor lexical indexes.

4. An important difference between the lexical and the syntactic indexes is that the former are ascertained on the basis of an identi-

⁶ *Lingua*, IV (1954-55), p. 108; V (1955-56), pp. 298-299.

fication with a full morpheme, e.g. *de-* in *deceive* is identified with the minimal sign (form + meaning) *de-* (in *dehorn*) = "depriving", whereas for the latter the identification has to be undertaken afterwards. In other words, not knowing yet whether the full morpheme *-us* (in *filius*) = "masc. + sing." must be identified with the full morpheme *-us* (in *hortus*) = "sing." and with the half morpheme (formal morpheme) *-us* (in *homo novus*), we nevertheless possess all the data for separating each one of them by a morpheme border. Before examining the possibility of their identification, I wish to make a few introductory remarks on the identification of morphemes in different positions.

This question confronts us again with the asymmetry of language. Hardly anybody would think of identifying the morphemes *bear* and *bare*, although they have the same phonemic make-up, but on the other hand no one would hesitate to do so in the case of *-s* in *cows* and *-en* in *oxen*, where the phonemes are completely different. In both cases we have suppletiveness on one plane (not necessarily complementary distribution) and identity on the other. The asymmetry, therefore, is to be ascribed not so much to language as to the attitude of man, for whom meaning is "more interesting" than form (I refer again to the quotation from Mrs. Langer, see above p. 84.) It is advisable to conform to usage in this respect.

The following rule may be set up: Two morphemes which have all their semantic minimums in common must be identified. This applies of course not to the formal morphemes, where the semantic make-up is zero, it holds good only for the full morphemes.

It should be noted that identification is often a prerequisite for the establishing of a morpheme border. The procedure for breaking down a sequence into morphemes largely works by way of identification, and thus many morphemes have been identified already. To this group belong among the formal morphemes the lexical indexes, and among the full morphemes many of the recurrent morphemes, but none of the rest morphemes. In all cases where the identity has been established already (with the exception of the "empty morphs"), complete semantic identity was a condition. As said above, the same criterion can be employed for all morphemes

which are left. Let us take the ending of *oxen*. We start the analysis of this word by setting up the purely semantic proportion *cow* : *cows* = *ox* : *oxen* (Chapter II). So we formulate the correspondences *ox* = "ox + sing.", *oxen* = "ox + plural". The element *ox* is repeated in exactly the same phonemic shape, which allows for a morphemic dissociation, and *-en* is the remainder. Here we consider the semantic identity of *-en* and *-s* sufficient for identifying them: they are phonemic allomorphs of one "morpheme".

I shall illustrate a complicated variant of this case by means of a Dutch example. In this language there is more than one suffix for "plural": *-en* in *bomen* "trees", *-s* in *lepels* "spoons". They are both recurrent morphemes, and the criterion seems to prescribe their identification. However, it happens, though very rarely, that a word can take both suffixes, e.g. *appelen* or *appels* "apples". Both forms are equally common, I am not able to find even the slightest difference in their application, neither semantically, nor stylistically.⁷ We have here one of those cases where "one might insist that free variation actually exists between the forms".⁸ What Nida says about such forms, however, does not apply to the Dutch example: "But the semantic difference between them derives from the whole pattern of the language; and even though the forms may have the same frequency statistically, they can and will nevertheless be different in meaning, if this is measured by their distribution in terms of the language as a whole" (*ib.*). There are no doubt instances where no measurement would preclude a semantic identification, and then the general "assumption that there are no actual synonyms" (*ib.*) should suffice: any time a speaker uses one of these forms, he makes a choice, no matter how unconsciously. The fact that the occurrence of a form is the result of a choice sets this form against some other form, i.e. lends it some kind of feature. This feature pertains to the semantic plane, because

⁷ For this reason I have chosen the example from the Dutch material. I am even inclined to say, instead of "Dutch", "one type of Dutch", although I do not know of other types where the situation is different. Only in this way the rightness of the starting point is beyond doubt.

⁸ Nida, "The Identification . . .", p. 432, n. 34.

the attitude of the speaker is involved, although not towards the things meant, but towards the way of expressing himself. A replacement of one form by the other in a given utterance does not change its applicability to reality, but either of them may be selected for reasons of rhythm, meter or rhyme. I propose to call such semantic properties "stylistic features".

Stylistic features are not semantic minimums and should not influence the ascertainment of morphemes. In the given instance, the "morphs" *-en* = "plural" (in *bomen*) and *-en* = "plural" + stylistic feature (in *appelen*) must be identified, despite the difference of their semantic make-up. An example of a somewhat different nature is presented by the Russian suffixes *-oj*, *-oju* for the instrumental singular, e.g. *piloj*, *piloju* "with a saw". A "real" difference of meaning is difficult to find, they are interchangeable without any consequence for the applicability to reality, but *-oju* is definitely less frequent than *-oj* in the modern language, so that here the occurrence of *-oju* indicates a certain selection by the speaker, which cannot be said when *-oj* is used. The choice of *-oju* reflects an attitude of the speaker, and this, being formally expressed, compels one to assume a relevant element of meaning, a stylistic feature. We can set up the correspondences: *-oj* = "instr. sing.", *-oju* = "instr. sing." + stylistic feature. If we considered stylistic features semantic minimums, we would have a right to isolate a morpheme *-u* = stylistic feature. Their special status prevents us from doing so. The "morphs" *-oj* and *-oju* are formal and semantic variants of one "morpheme".

Besides their stylistic function, the Dutch plural suffixes *-en* and *-s* play in certain words a more important semantic role. There are two homonyms *patroon*, one meaning "cartridge", the other "employer". In the plural they are formally distinguished as *patron-en* and *patroon-s*, respectively. The suffixes do not only contribute the meaning "plural", but they also indicate which of the two homonyms is meant. They share this property with the lexical indexes, but, unlike the latter, they are not "empty morphs". They have, what we may call, a "lexical function". This is an other kind of semantic feature which does not affect the morphemic

analysis: the same morpheme *-en* appears in *patronen* and *bomen*, although the suffix has only in the former combination a lexical function. Consequently, the morpheme list of Dutch contains a morpheme occurring in at least three phonemic realizations, /s/, /ə/, /ən/, and in three semantic realizations, (1) "plural", (2) "plural" + stylistic feature, (3) "plural" + lexical function.

The identifications treated thus far took place on the basis of a complete identity of the semantic minimums. However, an identification based upon a lesser degree of semantic similarity is not excluded. The identification rule can be extended as follows: Two morphemes *a* = "a" and *b* = "b" must be identified if (1) *a* and *b* are identical, (2) „a" consists of a part of the semantic minimums which constitute "b", and (3) they have a different distribution. An example is the morpheme *cow* = "number-irrespective cow(s)" (occurring in *cows*, *cowcatcher*), which is a semantic allomorph of *cow* = "number-irrespective cow(s) + singular" (in *the cow*). A difficulty is presented by those cases where a morpheme would in this way be identified with two mutually not identifiable morphemes, such as *sheep* = "number-irrespective sheep" (in *sheepshearer*), which stands in such a relationship to both the singular and the plural *sheep*.

For fear of overloading the terminological apparatus, I shrink from introducing the expression "archimorpheme", although a comparison with "archiphoneme" seems to impose this term. If one does not like to list a separate entity, one must try to find additional arguments justifying the choice of one of the two possible identifications. In the case of *sheep*, the fact that "plural" often constitutes the meaning of a separate morpheme, while "singular" is always only a part of the semantic make-up of a morpheme, suggests the identification of the singular and the numberless form. Sometimes, however, one looks in vain for facts supporting either possibility. There are, for example, in English at least two homonymous prefixes *de-*, so that the word *decompound* occurs with the two discrete meanings of "compounding things already compound" and "resolving into constituent parts". With which of these shall we identify *de-* of *deceive*? The fact that it can be identified with either

was decisive for breaking up the word, but the choice will be arbitrary. Therefore meaningless *de-* must remain a separate entry in the morpheme list, no matter how we solve the problem of terminology.

For the purpose of identification the difference of distribution is sufficient: the morphemes may very well occur in the same immediate environments. Indo-European languages with a rich flexional system furnish many examples. In Latin, the suffix *-īs* of *in hortīs* carries the meaning "ablative" (compare *in hortōs*) and "plural" (compare *in hortō*). In the construction *ex hortīs*, however, only "plural" is left, for no other case than the ablative is admissible after *ex*. Extreme examples of the same phenomenon are the syntactic indexes, e.g. *-īs* in *ex Athēnīs*: their semantic make-up equals zero.

The preceding rule is also valid when the two morphemes occur in analogous positions. This means that the whole environment is the same with the exception of one contiguous element. Compared with *in hortīs*, where the suffix means „ablative + plural“, a construction like *in Athēnīs* is semantically poorer; there is no opposition to a singular. The only meaning of the suffix is "ablative", but this does not prevent the identification. Syntactic indexes, such as in *ex Athēnīs*, can be regarded as extreme cases of the application of this rule: the semantic content is reduced to zero.

In the syntactic indexes the entire semantic content of an element is replaced by a syntactic function. In the less extreme cases there is only a partial replacement of this kind; a part of the semantic minimums is still present, but it cannot be denied that in *ex hortīs* the suffix has also a syntactic function, for, from the suffixes meaning "plural", only *-īs* is admissible in this environment. The suffix *-ōs* with the same meaning, on the other hand, occurs only in other environments, such as *ad hortōs*. The difference of the syntactic functions makes us reject the identification of the two morphemes *-īs* = "plural" and *-ōs* = "plural". This is the first restriction of the rule that two morphemes which have their semantic minimums in common must be identified.

The second restriction concerns the cases where a morpheme

border interferes. It goes without saying that the French morpheme *au* cannot be identified with the group of morphemes *à l'*, in spite of their semantic identity.

It sometimes happens that the procedure forces us to draw a morphemic borderline in the form, while it fails to indicate where this should be done. The Dutch form *innemen*, composed of *in* "in" and *nemen* "take", has only one consonant between the vowels of the first two syllables. In a morphemic notation it would be engaging to follow the so-called morphological principle and to attribute the *n* to both morphemes, but this would be contrary to the phonemic facts. In such cases it is indeed quite immaterial on which side of the borderline we situate the phoneme; for practical reasons it seems recommendable to attach it to the second morpheme. The arrangement of the morphemes along the temporal axis makes it preferable to leave the initial parts of the morphemes intact: if there is a choice between two analyses, one resulting in two allomorphs with different final morphemes, the other yielding two morphemes differing initially, the former solution is the better one. There are often other indications at which place a cut has to be made. So, in the case of *houses*, a comparison with *dish : dishes* learns us that /iz/ is the ending morpheme. Elsewhere such indications may be lacking and the choice will be arbitrary, but this does not alter the fact that in general a linguistic analysis is not complete without the establishment of the borderlines between the "morphs". This is what I have tried to demonstrate in the foregoing pages.

5. Things are frequently more complicated than would appear from the above exposition. The meanings of a word like *rat*, referring to an animal and a man respectively, are discrete in the sense I have proposed in Chapter II, i.e. we cannot use *rat* in such a way that it has both meanings simultaneously, unless we make a pun. Are we forced, then, to regard them as entirely disparate and to list two homonymous morphemes? We clearly "feel" a connection between them, we would rather like to speak of "transferred meaning", but can we justify this? There is no doubt a semiotic relationship between two such meanings: the one "stands

for" the other. Evidently, sign function plays an extensive role in human culture; it exists not only in language. A sign is linguistic when the significans is a linguistic expression, a sequence or an arrangement of phonemes. In the case of *rat* referring to a man, however, the significans is a linguistic meaning ("a certain kind of rodent"). We cannot expect to ascertain a nonlinguistic semiotic relationship by means of linguistic criteria. When we examine two genuine homonyms, like *pair* and *pear*, we also find that they represent two discrete semantic minimums, but we do not regard the meaning of either of these words as "transferred". Why we do so, however, is a question which cannot fully be answered by a linguist as such.

Of course, I do not have in view here the metaphorical use of a word, which is always recognizable by the incapability to serve for direct pointing (see above pp. 89f). Nor do I include here all those cases where the original meaning is no longer attested in the language. If we are told that the meaning of *hand* has developed from an "original" meaning "catcher", we do not consider it for that reason synchronistically a "transferred" meaning.

From a synchronistic point of view, it is even impossible to decide with purely linguistic arguments, which of the two meanings of the word *rat* is "original" and which is "transferred". Therefore it is better to speak of two "concurrent meanings", and we may define: Concurrent meanings are semiotically, but not linguistically, related meanings corresponding to the same linguistic form.

Not only single morphemes, such as *rat*, but also combinations of morphemes, compounds or phrases, may occur with two concurrent meanings, e.g. *anteater*. The difference with the one-morpheme cases is that a linguist can here with his own methods distinguish between the "original" and the "derived" meaning, for the original meaning consists of a number of semantic minimums, whereas the derived meaning has only one. There are various relationships between two concurrent meanings. In the case of *anteater* the derived meaning is applicable to a special kind of ant-eating beings, any anteater (2) is an anteater (1), but not vice versa. This does not hold good for a *guinea pig*, which is evidently not

a kind of *Guinea pig*. Furthermore we have the metaphorical type, as in Dutch *rode hond* "German measles", which literally means "red dog". But all these types are linguistically on a par.

Another example of the same phenomenon confronts us with the question: does the expression *U.S.A.* consist of three morphemes or of one? If there are three morphemes, their meaning is just three letters, exactly as when one spells a word in which these letters occur in this order (such as *usage* or *refusal*), and the road leading from these letters to the words they are the abbreviation of, presents again a nonlinguistic symbolization. However, if we take into account only the phonemic starting point and the ultimate meaning "America", there is only one semantic minimum. Both approaches are linguistically sound. The description depends on whether we are willing to assume that the expressions *U.S.A.* and *United States of America* are indeed related, but since such a relationship lies outside the domain of linguistics proper, this science cannot supply the answer to the question. This case is in a certain sense the converse of the preceding: there are two linguistic forms, one of which is in an extra-linguistic sense the meaning of the other.

It is advisable for a linguist to take the nonlinguistic relationships for granted, as long as they are not contradicted by linguistic data. Thus we list a single morpheme for the word *rat*, but we add that it occurs with two concurrent meanings. Likewise we list a single morpheme /yuw/: when it appears with its primary meaning, it points to a letter or a sound; its secondary meaning is pertinent only when it is combined with certain other morphemes of the same type. In such a combination it is impossible to state what formal morpheme points to what part of the ultimate thing meant: the separate elements are stripped of their own meaning, there are no semantic minimums corresponding to each of them taken separately; together they are associated with one semantic minimum.

To sum up: for the purpose of morpheme identification, concurrent meanings must be interpreted as same. So we identify the name of the letter *u* and the first syllable of *U.S.A.* Likewise in the

case of a *suppositio materialis*: *book* in *Book is a word of four letters* represents the same morpheme as in *Read this book*.

A proper name, such as *John*, is applicable to different persons who need not have anything in common but their name. These persons form a semantic category kept together by the mere fact that they are handled in linguistic intercourse by the same word. Such a category differs from a category of things covered by a common noun, say *stone*, in that the factor constituting its unity is limited to the moments its members are the subject matter of speech, whereas all *stones* share a property under all circumstances, linguistic as well as nonlinguistic. This difference, however, is not fundamental from a linguistic point of view, and the realizations of the meaning of *John* are not discrete. On the other hand the meanings of *stone* in *a stone* and *Mr. Stone* respectively, are undoubtedly discrete (the semantic minimums are different), but nothing prevents us from regarding them as concurrent meanings, so that the two forms must be identified on the morphemic level.

The meanings of *Blackpool* (one semantic minimum) and *black pool* (two semantic minimums) cannot be interpreted as concurrent, for the forms are differentiated by the accent pattern, but this does not hold good for their constituents, so that we arrive nonetheless at a morphemic identification. The accent indicates that the derived meaning is pertinent, both for *black* and *pool*. In *blackbird* (as compared with *black bird*) the accent has the same function, but here its effect is limited to the element *black* (there are two semantic minimums in both expressions).

NOTE ON LARGER UNITS

In the present study units larger than the morpheme are not treated. The delimitation of sentences poses hardly a problem. Syntagmatically, a sentence is a part of the morpheme sequence marked by a certain intonation pattern. Paradigmatically, the distinct types of intonation patterns may be kept apart by the criterion of discreteness (see above pp. 53f).

The word is somewhat harder to ascertain. There exists an excellent operational definition. Its author is Greenberg.¹ It contains, among other criteria, the requirement that the possibility of inserting elements in a word be restricted. It shares with other definitions the point that any appeal to the native speaker's judgment is dispensed with. This is definitely a weakness. We illustrate this with an example taken from the Latin poet Ennius: *saxo cere-comminuit -brum*. Greenberg expects from his definition "that it provides a method for dividing a particular utterance into word units. We do not ask, as is sometimes done, whether "hand" is a word in English but whether, in the utterance "the hand is quicker than the eye", the sequence "hand" constitutes a word" (p. 67). So we seem to have a right to ask whether in the example the sequence *cere- comminuit* consists of one or two words. When, however, we would analyze the sentence *saxo cerebrum comminuit*, we would have to decide whether an insertion is possible between *cere-* and *-brum*. A linguist will usually wish to give a negative answer to this question. He can do so on the basis of one out of three possible assumptions: either (1) he regards the rules of traditional normative grammar as decisive and declares that Ennius' sentence is wrong and hence does not exist for the analyst, or (2) he recognizes the validity of statistical data and counts the

¹ In *Psycholinguistics* . . .

occurrences, or (3) he accepts the judgments of the natives as a source of knowledge. As to (1), its rejection needs hardly an argumentation, it is rather astonishing how often it is tacitly applied in otherwise structuralistic studies. Furthermore, view (1) fails to afford a solution in the case of *cere- comminuit*: if the sentence does not exist, it cannot be analyzed into words, either. As concerns view (2), it is questionable whether in recordings of *spoken* utterances the "normal" sentences really outnumber the "wrong" or "incomplete" ones. Even in modern novels the dialogues meant to be natural differ in this respect considerably from what one hears in a conversation; in this way normative grammar enters again through a back door. But it cannot be denied that normative grammar partly reflects the ideas native speakers have about their language, and on the other hand that the speakers' habits are often strongly influenced by normative grammar. A native will inform us that the arrangement of the elements in *cere- comminuit -brum* is stylistically colored, and he will also be able to produce a sentence with the corresponding neutral or basic arrangement. When the insertion of an element results in a marked construction, while the sentence started from is evaluated as neutral, the insertion is of the kind Greenberg and others call "impossible". We conclude that even *cere- ... -brum* is one word. It goes without saying that we can find in several ways indirect indications about the judgment of the speakers, and in the case of dead languages we are forced to do so.

Greenberg's operational definition is easily the most successful of all attempts made thus far. It is characterized, however, by the same one-sidedness which is noticeable in other parts of present-day grammar. The unity of the word is established on the basis of features of arrangement of the constituent morphemes, but these features are exclusively of a formal character. Since, however, morphemes have not only a formal, but also a semantic aspect, it would seem advisable to take into account also their arrangement in the semantic whole. Since this question falls outside the scope of this study, we must postpone the treatment of it to a later occasion.²

² See a preliminary exposition of this author's view in the paper mentioned in footnote 1 on p. 104.

NOTE ON DIACHRONISTIC LINGUISTICS

1. If it is true that a language consists of a number of coexisting idiolects, it is equally true that the history of a language presents a series of successive idiolects ("a language" in this sense is not a linguistically definable notion). There is no sharp distinction between coexisting and successive idiolects, for a change never affects the code of all speakers simultaneously, so that a newly appeared idiolect exists side by side with the one from which it has developed. It is a very common phenomenon that different generations have different idiolects.

Many scholars reject the idea that language evolves by jumps.¹ No doubt they are right, but only as long as they restrict themselves to the nonlinguistic aspects of language. However, when we study the code of a community defined by the identity of its members' idiolects, we must admit that a gradual change is excluded. Every normal, grown-up person possesses at every moment of his existence a linguistic system which allows him to interpret unequivocally any utterance. It is, at least partly, a system where everything is related to everything else. All speakers of an idiolect have a complete control of the systematic part of the code. This part consists of (1) the manner in which the continua of sound and meaning are parcelled out and (2) the relevant features of arrangement of the linguistic units and their distribution (these are not dealt with here). The units themselves do not belong to the systematic part of the code: they are not exactly the same for all speakers of a single idiolect. A speaker may acquire new lexical items or

¹ This idea is defended, e.g., by R. Jakobson, "Principes de phonologie historique" in Troubetzkoy, *Principes*..., pp. 315-336 (reprinted from *TCLP*, IV, 1931, pp. 247-267): "les changements phonologiques procèdent par bonds" (p. 318).

forget them without changing his idiolect. He may also increase the number of phonemes by filling a gap, and this will not change his system, either. Real changes of the system occur only when an area of the continuum is split up by the introduction of a new phonemic or semantic borderline or when two minimum units merge into one. And this can happen only suddenly, for otherwise the speaker would temporarily be without a serviceable language. As a matter of fact, I believe that changes of the code are rare: a newly acquired distinction will hardly ever reach the degree of perfection required by the demand of discreteness. Though I have observed many speakers (and among them myself) who tried very hard to get rid of their dialect habits and to acquire the richer phonemic system of literary Dutch, I have never met one for whom the new distinctions were more than a mere artificial appendix to their natural system, no matter how perfectly they succeed in imitating the speech of the more privileged.

I shall work out a simple example of linguistic change. At a certain period (A) in the history of Old-Slavic, a [k] before front vowels changes into [c']. The phonemic system remains unaltered, for [c'] is nothing but a new realization of the phoneme /k/. At a subsequent point (B) a new generation, learning the language, acquires *k* and *c* not as two variants, but as two separate phonemes. To them it is quite fortuitous that these phonemes are in complementary distribution. Finally, at a point (C), foreign words are borrowed which supply the fortuitously absent combinations, such as /kesarjb/ from Greek *καῖσαρ*. The transition (A) is not phonemic, and thus gradual; it must have covered a period of some duration. The point (B), however, is marked by a sudden change: the difference between [k] and [c'] is either relevant or irrelevant, never halfway between. It goes without saying that this change (B) is not simultaneous for all members of the language community, it may even affect only a part of them. As said in the preceding paragraph, I am convinced that this point is mostly to be looked for in the period of language learning. The child pronounces [k] and [c'] in the same way as the adults he imitates, and he uses them in the same positions, but the system undergoes

a change in that these sounds are interpreted as phonemically distinct. This is an indispensable requirement for the borrowing of the group [ke] as distinct from the native [c'e]. Thus a borrowed form with [ke] is an indication of a *preceding* change in the system. Some scholars let the points (B) and (C) coincide in the description, but this view seems to present the development as too mechanical: it can hardly be accepted that the borrowing of a few words has such a far-reaching consequence for the language system. In my opinion, the point (B) is the only functionally relevant moment of change.

At an earlier time the Gothic word *kaisar* was borrowed by the Slavs as /česarjb/. Traditional historical grammar teaches that the phonetic law "k before a front vowel changes into c" still worked at this time, while it was extinct when /kesarjb/ appeared. The functional approach compels us to formulate the phenomenon otherwise: [k'] before a front vowel was first understood as an unusual realization of a phoneme /k/ (whose area extended from [k] to [c']) and automatically replaced by its common realization in that position. Later a similar foreign [k'] was interpreted as a realization in an unusual position of a phoneme /k/ (covering an area sufficiently distant from /c/ to be perceived as discrete). In both cases the foreign word served as an input for a kind of experiment which we can accomplish for living languages also in an artificial way.

Proof of a potential opposition is sometimes furnished by a form which appears in a language without any influence from outside. I quote Jakobson: "In Russian the use of the opposition 'hard ~ soft consonants' for grammatical alternations (*rv-ú ~ rv'-óš*, *vr-ú ~ vr'-óš*) calls into being a new pair *tk-ú ~ tk'-óš* and introduces a new phoneme, the soft *k'*, which formerly was a mere positional variant of the phoneme *k*."² I would prefer also here to regard [k'] before front vowels as relevantly palatalized, and [k] in other positions as relevantly nonpalatalized, although the opposition /k ~ k'/ had a zero functional yield. I draw this con-

² Jakobson, "The Phonemic and Grammatical Aspects....", p. 17.

clusion from the language's obvious capability of taking up a form like /tk' oš/. If one verb form were able to alter the phonemic system, it would indeed seem as if this system existed only in the imagination of the linguist. What happens with the appearance of forms like /tk'oš/ can only be that an already existing opposition gets a functional yield.

A merger of two phonemes into one takes place when a relevant border between two phonemes is not noticed as such by a younger generation, so that two areas of the sound continuum coalesce.

One phoneme is split into two phonemes which were formerly its stylistic variants, when a relevant borderline shifts along a dimension. In a position between vowels we may find, for example, a dimension [d — δ — j]. Before the change the borderline may have cut it between [δ] and [j], [δ] being a variant of /d/ occurring in slovenly speech. After the change the line must be drawn at some other place, e.g. between [d] and [δ], so that a same word is split into two homonyms (or semantically closely related words), the one with /d/, the other with /j/. The semantic difference between the two forms is then initially a question of style, and may subsequently develop into a difference in lexical meaning. Words occurring only in solemn speech will have only the /d/-form, while other words are so common in careless everyday talk that only the /j/-form subsists. This is approximately what has happened in Dutch.

2. Recently there appeared a very stimulating book on linguistic change by Coseriu.³ The author takes up the well-known contention of Humboldt that language is not an ἔργον, but an ἐνέργεια: "Realmente, y no en algún sentido metafórico, el lenguaje es actividad, y no producto. [...] para recordar una distinción aristotélica, una actividad puede considerarse: a) como tal, κατ'ἐνέργειαν; b) como actividad en potencia, κατὰ δύναμιν; y c) como actividad realizada en sus productos, κατ' ἔργον" (p. 25). In the present study language is approached from the side of

³ E. Coseriu, *Sincronía, diacronía e historia. El problema del cambio lingüístico* (Montevideo, 1958).

speech κατὰ δύναμιν, which is, according to Coseriu's definition, the speaking knowledge: "El hablar κατὰ δύναμιν es *saber hablar*" (*ib.*). This way of tackling the problem is in accordance with Coseriu's remark that "siendo ἐνέργεια en el sentido humboldtiano y aristotelico, el hablar es idealmente anterior a la 'lengua' [...] Históricamente, en cambio, la 'potencia' es anterior al 'acto'" (p. 26).

I do not follow Coseriu, however, in all his arguments. He declares that it is illegitimate to create the concept of language by definition, since languages belong to experience and "como enseñaba Kant en su pequeña *Lógica*, 'los objetos de experiencia no son susceptibles de definiciones nominales'" (p. 15). This may be true, but what we need first of all when we want to treat language in a scientific way is a *definitio realis*. Moreover, it is not certain that a language in the traditional sense is more an object of experience than a language at a given moment, "un *estado* de lengua", as Coseriu says. In this connection Coseriu speaks of "un error de perspectiva que se manifiesta fundamentalmente en la identificación - explícita o implícita - entre 'lengua' y 'proyección sincrónica'" (p. 8), and rejects the introduction of the notion "idiolect" as a way of solving the apparent discrepancy between the coherence of the language system and the possibility of changes. He remarks (p. 36) that an idiolect, an individual language, is strictly speaking no language at all, because a language presupposes at least two speakers. This remark is very interesting. It implies in my opinion that an investigator working with a speaking informant is unable to gather data on a language as long as there is no interpreter around (or unless he assumes himself this role). A speaking informant does not produce anything but a text, and this will yield data on the language only when it is interpreted. But we can avoid this difficulty by working with an interpreting native. When a person is confronted with an utterance, we obtain a normal speech situation: the idiolect of the producer of the utterance is of no importance for the establishing of the listener's idiolect.

An idiolect can be the object of a scientific analysis because it can be demarcated, a language in Coseriu's sense (i.e. taken as an

object of experience) fails in this respect. It is possible to describe all linguistic facts of the whole world and of all times exhaustively, both in their synchronistic and diachronistic aspects, without employing the traditional notion of "a language". We need this notion, however, when we study the interplay of linguistic and nonlinguistic factors, such as in culture or political history, but for this purpose a linguistic delimitation of the object can be dispensed with.

As a consequence of his conception of language, Coseriu contends that "los cambios se manifiestan en la sincronía" (p. 66). He adduces a number of examples where a language system already contains the germ of future changes. He sees these germs in the possibilities of selection, and it depends precisely on selection whether or not a change actually takes place. One of his examples (p. 73) is the comparative degree in Latin and Spanish. In Latin the desinential form prevailed, but in some cases a periphrastic form with *magis* was obligatory. The latter form also occurred as a facultative variant with the adverbs, although rarely (*magis audacter*, Cicero). So there was a possibility of selection and this happened to cause a change: "Sólo después de una larga selección *magis* fue el único modo comparativo admitido [in Spanish]." If we regard, however, the language stages in question as a number of simultaneous and successive idiolects, we may describe the development as follows: for certain idiolects (among them Cicero's) *audacius* was normal and *magis audacter* stylistically colored. At a certain moment there was a tendency to prefer the marked variant for expressive purposes, and then it was inevitable that a new generation interpreted the more frequent form in their teachers' speech as the unmarked, and viceversa. There may be an intermediate stage (but not necessarily so, as Nida contends): "When two fluctuating forms are shifting their status – when the originally more frequent form is growing less frequent and the other is climbing up – there is undoubtedly a point at which the two are equally in use." ⁴

At any rate the germ of the change is not the possibility of

⁴ Nida, "The Identification . . .", p. 432, n. 34.

selection between two forms, but rather between two interpretations of the forms.

Perhaps the idea will become clearer when it is illustrated by a phonological example. In Old Russian there was a word written *teti* "to cut", which changed phonemically from /tɛti/ to /t'ati/. It kept its distance from the nearby words /teti/ "to beat" and /tati/ "thieves". (I disregard the accentual differences.) I believe in such cases that the pronunciations immediately before and after the change were exactly the same: [t'äⁿti], but the elder generation felt timbre + nasality of the vowel as the distinctive property, while in the new idiolects this function was taken over by the palatalization of the consonant. Before the change a redundant feature (palatalization) became more and more prominent, after the change a redundant feature (nasality) gradually disappeared.

If this description is right, it is the realization of the system, rather than the system itself, which furnishes the possibility of alternative interpretations. The system certainly stimulated the change by presenting two structural particularities: (1) /t/ occupied an area including [t] and [t'], (2) the opposition palatalized ~ nonpalatalized existed already in /l' ~ l/, /r' ~ r/, /n' ~ n/. But the matter was settled by the fact that the area [t'] was commonly utilized for the realization of /t/ before front vowels. In this light, nothing prevents us from seeing a language stage (group of same idiolects) as a homogeneous whole.

On the other hand, changes which are foreboded in a preceding stage of the language system are not always changes in the strict sense of the word. Some of them may be called, with Milewski, derivations (such as gap-filling), others affect only the realization of the system. Jakobson gives examples of the latter phenomenon.⁵ The first one concerns the change in Russian of [ɛ] into [e] before palatalized consonants, while both sounds remain variants of one phoneme. What happens here is that a previously not utilized part of the /e/-area (not an empty pigeonhole in the system!) is occupied by a new variant.

⁵ Jakobson, "Principes de phonologie historique" . . . , pp. 317f.

3. In this chapter I have touched on some consequences of the theory of linguistic units expounded in this study. I wish to make it clear that these remarks have a bearing only on the descriptive side of diachronistic linguistics, not on its explanatory aspect. So I leave undecided whether Coseriu's possibilities of selection or Martinet's internal and external sources of unbalance are more to the point (in so far as they are not one and the same thing). The explanation of linguistic development forms a fairly new domain of inquiry, the exploration of which is actively undertaken by structuralists. In this connection the work of Martinet deserves to be mentioned in the first place.⁶

⁶ See, e.g., A. Martinet, *Economie des changements phonétiques* (Berne, 1955).



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